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PERIMETRY OF THE LOCALIZATION OF SOUND.

BY

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PART II.

CONTENTS.

The first part of this investigation was published under the same title in *The University of Iowa Studies in Psychology*, 1905, IV, 1–45. The object of Part I was to determine the accuracy of localization in various representative directions. The present research is an effort to study in detail some of the numerous problems to which those experiments gave rise. The following is a report of five series of experiments which were made during the academic years 1904–06.[1]

SERIES I.

Sensibility to Sound in Different Directions.

Problem, Method, Apparatus and Observers.

One of the most frequently recurring observations in the experiments of Part I was the fact that a sound of uniform intensity and at a constant distance from the observer seemed to be nearer and correspondingly louder in some directions than in others. The problem then was to determine the extent of this variation in apparent intensity and apparent distance of an objectively uniform sound.

How can this subjective variation be measured in a definite way? Intensity of sound varies in some ratio, inversely with its distance; we estimate the distance of a sound by its intensity and the intensity is interpreted partly in terms of distance. We want to measure, in a selected group of directions, the actual variation in intensity that is necessary to make the sound seem uniform to an observer. Assuming that the sensibility or threshold of hearing remains fairly constant during the time of the measurements, we may first measure the acuity of hearing in representative directions in which apparent changes of intensity have been noticed. The differences in the thresholds of

[1] The writer wishes to express sincere gratitude to Prof. C. E. Seashore for suggesting the original problem in Part I, for his many suggestions of new problems and methods of experimentation throughout the entire investigation, and for the time he has given as observer in several series of experiments. Acknowledgments are also due to all the observers who participated in the experiments.

hearing among these directions may then be regarded as measures of the differences in apparent intensity.

For this purpose, apparatus was needed to produce a sound variable in intensity according to definite units but uniform in all other respects. An electric fork of 100 v. d. was driven by a current of three amperes and three volts, which were kept constant throughout the experiments. A shunt from the fork was completed through the primary coil of Seashore's audiometer,[1] of which the secondary coil was connected with a telephone receiver. The audiometer is an instrument devised for the purpose of controlling and measuring the intensity of sound. The essential part of the audiometer is a primary and a series of secondary coils by which the strength of the current and consequently the intensity of the sound can be varied. The scale of intensities which rises from one to forty, is based on the psychophysical law so that the ratio of any two successive increments on the scale is psychologically the same.

The apparatus was distributed in three rooms. The fork was mounted in the battery closet, the audiometer operated by the experimenter was in the measuring room, and the receiver was mounted in the observing room which was moderately lighted by incandescent lamps, and practically sound-proof—a condition necessary for successful experiments of this kind. The receiver was mounted on a tripod and, in order to avoid resonance, was insulated from the iron support by heavy felt. A pasteboard tube, six inches long and two inches in diameter, lined with felt cloth, was attached to the face of the receiver for the purpose of directing the sound toward the observer. Throughout the tests the receiver was kept in the same position and at a constant distance from the observer, namely one meter from the center of the head.

The observer was guided in keeping the proper position by a ring of wire suspended from above and hanging freely about the head. In finding and keeping the various positions the

[1] For the original description of the audiometer see, Seashore, "An Audiometer," Univ. of Iowa Studies in Psych., 1898, II, 158–163. A briefer description can be found in the Univ. of Iowa Studies in Psych., 1905, IV, 48–49.

observer turned to the desired position in each case and was guided by labels on the walls.

The threshold measurements were made in a series of directions in the right half of the horizontal plane through the aural axis at points 15° apart. The points were, 0° front, 15° right front, 30°rf, 45°rf, 60°rf, 75°rf, 90°r, 75° right back, 60°rb, 45°rb, 30°rb, 15°rb, and 0° b.[1]

In determining the threshold of hearing we may proceed in two ways, following the method of minimal change; we may begin either with a subliminal sound and increase it until it is just perceptible, or with a supraliminal sound and decrease it until it is just not perceptible. In the former case we get a determination (*To*, threshold over) just a little above the determination obtained in the latter case (*Tu*, threshold under). The average of the two is regarded as the threshold. For the present purpose, however, it is better to use only one of the determinations and thereby have the advantage of a simpler computation and evaluation of the results. *To* is preferable to *Tu* because it is easier to determine the appearance of sound than its disappearance.

The measurement proceeded as follows. The observer comfortably seated on a stool in the observing room, held in his hand a strap key which was connected with a sounder and battery in the measuring room. All communication between the observer and the experimenter was by means of signals through the sounder and the receiver. The experimenter began by giving a loud sound in the receiver as the signal for starting. After an intermission of two or three seconds he started at a subliminal point, usually from five to seven units below the threshold, and increased the sound at the rate of approximately one step a second until the observer heard it and responded by a tap on his key. The step on the audiometer scale at which the response occurred was then recorded. This constituted one determination. In the same manner ten determinations were made in succession for a given standard direction. In

[1] Cf. Fig. 2, page 6, in Part I.
In this system of designating directions 0° front is directly in front, 0° back is straight back, and 90° right is opposite the right ear.

order to eliminate from the results the possible disturbance coming from the change in position on the part of the observer, in passing from one direction to another, the first three measurements at each direction were not recorded. After ten records had been made in one direction the experimenter again gave a loud sound through the receiver. The observer then turned to the next position and signalled as soon as he was ready to begin again.

The observer also had the right to throw out of the record any trial in which he had anticipated or delayed his response or in which some other disturbance had occurred to invalidate the measurement. But this did not occur often when conditions were normal. The observer had to indicate immediately his desire to discard a measurement, by signal to the experimenter. A complete record consisted of twenty determinations in the double fatigue order for each one of the thirteen chosen directions. The time required for taking such a record was approximately forty-five minutes.

Records were obtained from eight persons, N. C., D. S., C. E. S., E. A. J., N. B., E. G. Q., R. W. S., and H. S. B. Two of these, N. C. and N. B., are women. C. E. S. and D. S. were experienced observers and the others were students in the technical laboratory course. After two or three records had been taken on each observer, it was noticed that they fell into two distinct types according as the threshold for front was higher or lower than for the back. In one type the threshold for front was higher than for the back, and in the other type the reverse was true. One representative of each type was chosen (N. C. and D. S.), and a series of ten records was obtained from each one of these two. In all, thirty-five records were obtained from the eight observers, making 700 determinations for each direction, or 9,100 in all. After these tests, N. C. and D. S. each made three more records using only one ear.

Before the regular tests were begun the threshold was found for each ear separately in the case of every observer, as a large difference between the two ears might affect the records. An observer never made more than one record a day. They were at the same hour on successive days, barring a few exceptions.

The order of the directions was reversed for successive records; instead of beginning all the records in front, each alternate one was begun in the back so as to make the conditions as uniform as possible for all directions. The observer was alone in the room, which was thoroughly ventilated before each test.

Binaural Threshold.

Method of recording—The following is a typical record in the statistical form in which they were originally made during the course of the experiments. The numbers are the readings on the audiometer scale for the separate determinations.

Specimen Record.

	0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
	22	20	18	16	14	14	14	13	12	12	13	14	15
	22	20	17	16	14	13	13	12	14	13	14	14	15
	19	20	17	16	14	14	12	12	12	13	15	14	15
	21	19	16	16	15	13	12	12	14	13	15	15	15
	20	19	15	15	15	12	13	13	13	13	14	15	14
	21	19	17	16	15	13	12	12	13	13	13	15	15
	20	19	17	15	15	14	13	12	13	14	14	15	16
	20	18	16	15	15	14	12	12	12	13	14	15	15
	20	20	15	15	15	14	12	12	13	13	15	15	16
	20	18	17	15	15	14	14	12	13	13	15	15	15
m. v.	.8	.6	.8	.5	.4	.6	.7	.3	.5	.2	.6	.4	.4
	Second half.												
	17	15	16	13	13	12	12	13	13	14	14	17	20
	17	17	16	14	13	14	14	12	13	14	14	14	20
	18	16	15	14	13	13	11	12	12	13	13	15	17
	18	16	14	14	13	13	12	13	14	12	14	15	16
	16	16	16	13	13	14	12	13	13	14	14	16	16
	17	16	16	14	13	14	12	13	13	13	14	14	14
	17	16	15	15	13	13	11	12	14	13	15	17	17
	17	15	15	14	13	13	13	11	13	13	14	15	17
	19	15	16	13	12	14	12	13	14	13	14	15	16
	17	15	15	15	14	12	12	12	13	13	15	15	15
	—	—	—	—	—	—	—	—	—	—	—	—	—
	18.9	17.5	16.2	14.7	13.9	13.4	12.4	12.3	13.1	13.1	14.1	15.0	16.0
m. v.	.6	.6	.6	.5	.2	.6	.6	.6	.5	.5	.4	.8	1.4

The curves—Instead of giving all the records in this form, the results may be represented more advantageously and clearly in graphic form. Figures 1 and 2 contain the records of the two observers from each of whom ten records were obtained. Fig-

ure 3 presents the composite curves. Each one of the light curves represents the averages of all the determinations made at

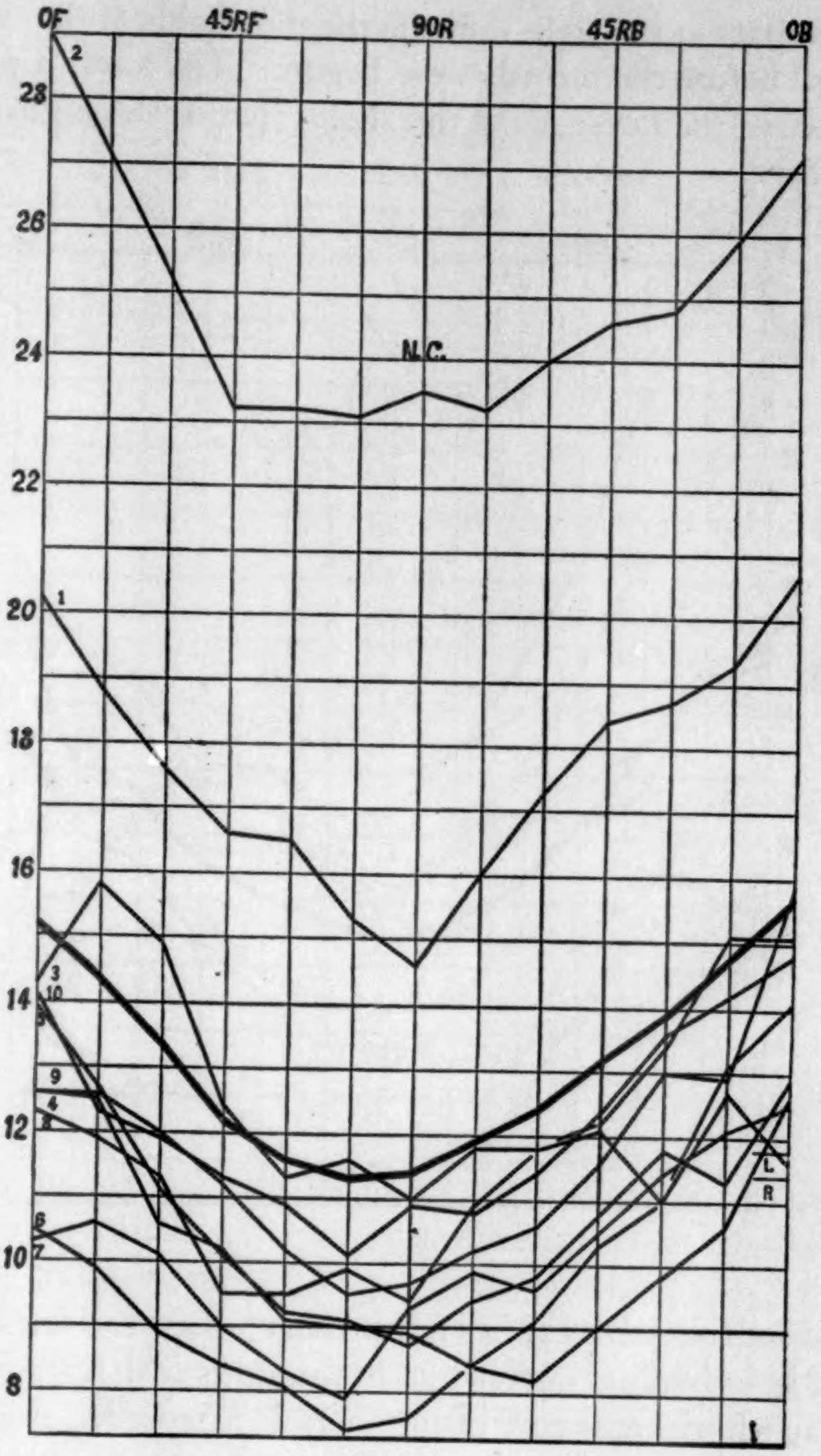


FIG. 1

each direction in one record. The heavy curves are the average curves. The numbers at the top are the designations for the

directions. The large numbers on the left are the steps on the audiometer scale, and the small numbers at the beginning of each curve indicate the order in which the records were made. The two bars at the right indicate the thresholds of the two ears obtained before the records were begun. The lower a point is in the curve the lower is the threshold; that is, the keener is the sensibility.[1]

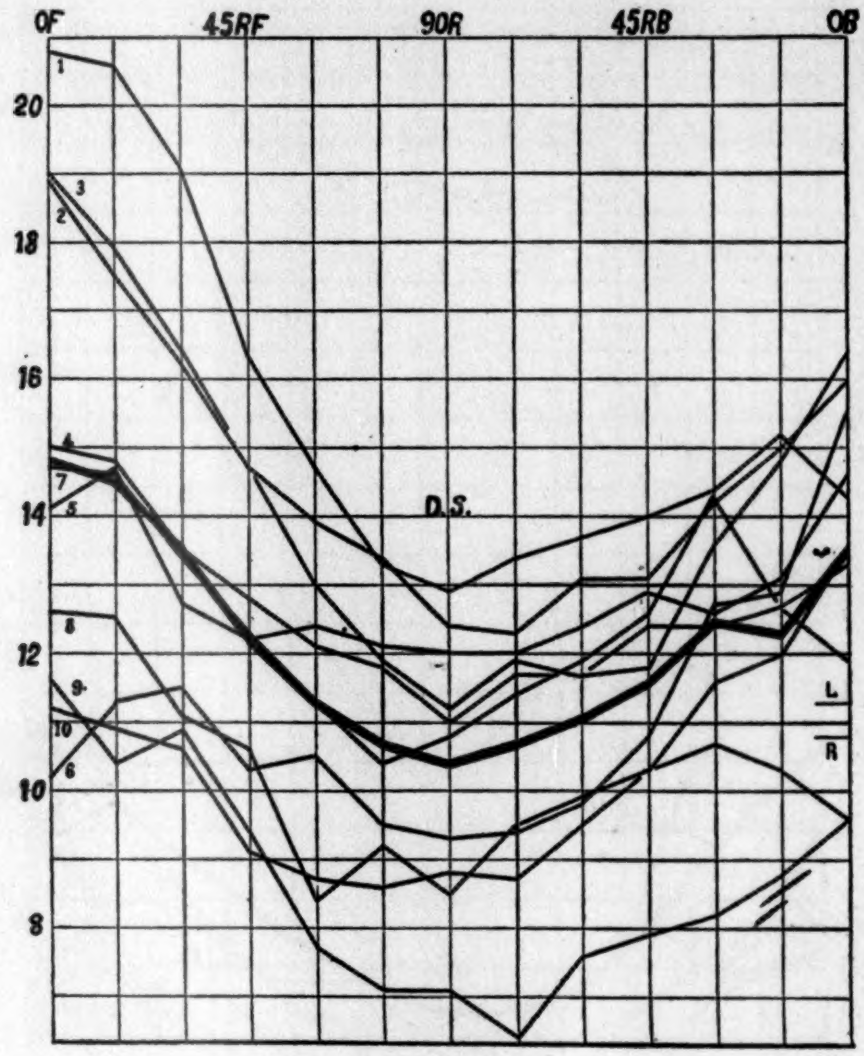


FIG 2

The individual records of the other observers are not presented in individual curves but in the statistical form of Table I. Each horizontal row of numbers is a record. Each num-

[1]The first two curves in Figure I are considerably higher than the others. This is due principally to the conservative standard of responding which the observer had adopted. She did not respond until she had "verified the hearing of the sound." In the latter records a less conservative standard was adopted.

ber is the average of the twenty measurements. The upper part of Figure 3 is a composite curve based upon this table.

TABLE I.

0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
						C. E. S.						
15.6	14.4	13.9	11.8	11.2	10.4	10.0	11.1	11.2	11.9	15.3	15.5	16.5
15.1	14.6	12.7	12.3	11.2	10.5	10.1	11.0	11.5	12.2	13.8	14.5	16.5
						E. A. J.						
20.5	18.5	14.9	13.6	11.7	10.5	10.2	10.8	10.8	12.1	13.8	15.3	15.3
16.1	14.7	14.0	13.1	11.4	10.2	10.1	11.3	11.3	11.2	14.2	14.7	15.3
						N. B.						
13.2	13.1	12.2	11.6	10.9	10.1	9.7	10.3	11.0	12.6	13.6	13.8	12.7
21.7	21.5	20.9	18.7	17.6	15.9	15.7	16.8	16.8	18.6	19.6	19.2	20.2
20.1	20.0	19.5	17.5	17.0	15.6	14.5	15.8	16.9	18.4	18.9	18.2	17.9
						E. G. Q.						
29.3	28.6	27.3	26.3	25.1	24.5	24.2	25.3	25.6	26.6	27.4	28.1	27.7
27.3	27.3	26.3	25.4	23.8	22.3	22.8	22.9	24.1	24.8	26.5	27.5	28.0
32.3	33.0	32.3	30.2	30.2	30.0	29.5	30.4	31.1	31.9	30.0	31.5	34.6
						R. W. S.						
20.4	18.7	16.0	14.9	13.3	12.7	12.8	13.5	13.0	14.2	15.9	17.2	17.6
15.0	15.6	15.1	14.7	13.8	13.2	12.9	13.6	14.6	13.8	14.9	14.8	14.5
						H. S. B.						
24.3	22.3	20.9	20.1	20.2	18.4	18.0	18.2	20.6	21.4	22.1	22.8	24.2
17.7	14.4	13.7	12.1	10.9	10.9	10.7	11.7	12.9	13.5	16.3	19.9	20.6
26.6	26.0	24.4	23.8	23.0	22.5	22.3	23.7	25.0	25.8	26.9	28.6	27.2
						Averages.						
21.0	20.2	18.9	17.8	16.8	15.8	15.6	16.4	17.1	17.9	19.3	20.1	20.6

The mean variation for these records is not given because it is relatively small in all the measurements. The mean variation in the sample record above is representative.

Data in the curves—(a) The region of keenest sensibility is at the side, at 90°r. (b) Front and back are considerably less keen than the side. (c) There is a difference among the observers in regard to the acuity in front and in the back, which divides them into two distinct types: those who have greater keenness in *front* and those who have greater keenness in the *back*. (d) The remaining directions occupy intermediate positions so that

the curves are free from breaks and present a relatively smooth appearance. The curves approach the shape of a flattened U.

If we compare the curves of Figure 3 with the other curves, we notice at once the general prevalence of these features. Although there is no absolute guarantee that the stimulus from the fork remained constant through the tests, there is every reason to believe that it did remain practically constant. We may, at any rate, safely assume that there were no serious changes

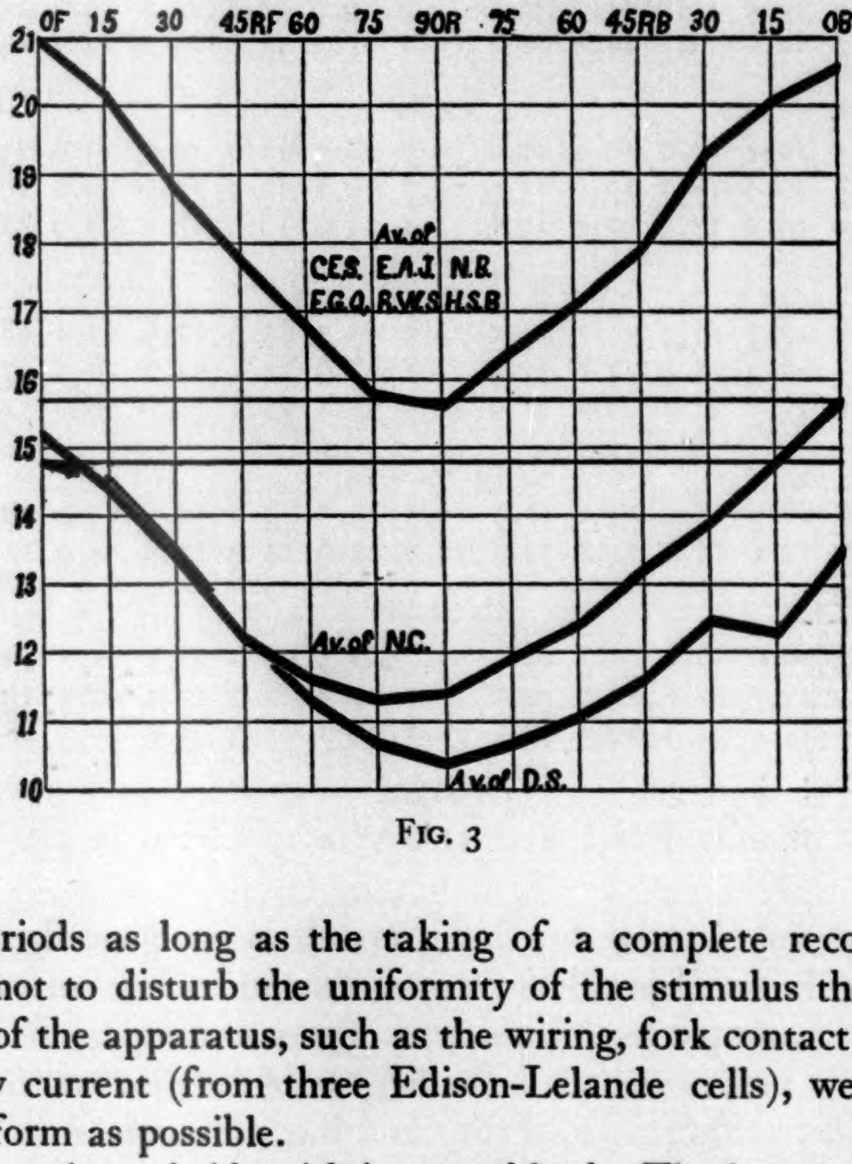


FIG. 3

for periods as long as the taking of a complete record. In order not to disturb the uniformity of the stimulus the accessories of the apparatus, such as the wiring, fork contact and the battery current (from three Edison-Lelande cells), were kept as uniform as possible.

Comparison of side with front and back—The keenness of the side as compared with front and back may be expressed in a ratio. In the average curve of N. C., Figure 3, the lowest, i. e., the keenest, point at the side is 11.3 and the average for front and back, 0°f and 0°b, is 15.5, giving the ratio 11.3 to 15.5 or

.73. The ratio of the average curve of D. S. is .72 and for the average curve of the other observers .75. It seems then that irrespective of the absolute thresholds, without regard to the relative keenness or obtuseness of hearing, the ratio is constant, being approximately 3 to 4. Whether the curve is higher up in the scale or lower down, its general form will be the same. See for example the individual records in Figures 1 and 2 and in Table I. This undoubtedly finds its explanation in the fact that the graduated scale of intensities on the audiometer is based on Weber's law, making the increments on all parts of the scale equally perceptible.

Comparison of threshold with "distance" tests—In Part I[1] a series of tests was made to determine the apparent distance of a sound of uniform intensity and distance in the same group of directions for which the threshold has been measured. It was there found that the same sound was estimated nearest at 90°r and farthest away in anterior and posterior directions. At 90°r it was estimated to be at a distance of 38.3 inches, while at 15°rf it was estimated to be 44.4 inches, and at 15°rb, 44.6 inches.[2] Thus the curve of the distance tests agrees with the threshold curves. The threshold is lowest at 90°r where the sound seemed nearest; and it is highest in the anterior and posterior directions where the sound seemed farthest away.

Introspections—The following introspective notes are quoted as they were written by the observers immediately after each test. They express characteristic observations and experiences of the observers during the experiments. Unessential parts and repetitions in successive notes are omitted.

D. S. 10.00 a. m., Oct. 25, '04.

Had my eyes closed. Have a cold in my head. Mind wandering was rather disturbing in the first half. Felt quite comfortable and was not particularly wearied at any time. Sometimes it seemed that I could hear the sound all the time and that I responded when it seemed to become louder. At the point 0°b the sounds seemed to be 15° or 20° to the left of 0°b. There was no tendency toward rhythmic response.

[1] See Part I, 20–23.

[2] 0°f and 0°b seem to be exceptions; they were estimated to be nearer than 15°rf and 15°rb.

D. S. 10.00 a.m., Oct. 27, '04.

Still have quite a severe cold in my head and occasional singing in my left ear. Noticed especially the rushing of blood in the head and the heart beating. Sometimes the sound seemed to come in pulsations corresponding to the heart beats but I could not determine whether they coincided. Could not notice any qualitative differences in the sound from different directions. Sometimes it seemed that the source of the sound was nearer at the side than in front or in the back. I do not think that my standard of certainty in responding changed noticeably during this test. My attention varied considerably. About half of the time it was mostly passive. Had my eyes closed because I feel that I can then pay attention better.

D. S. 10.00 a.m., Oct. 29, '04.

I anticipate that the thresholds in this record are lower than in the preceding ones because I seemed to attend better to the stimuli, and my cold is considerably better. At the side the sounds usually came in pulsations corresponding to the heart beats felt in the ear, and the distance of the receiver seemed less at the side than in front or in the back. It seems also that somehow it was easier to hear the sounds from the side, that is, I feel sure more quickly of hearing the sound there than at the back or in front.

D. S. 10.00 a.m., Nov. 1, '04.

Had eyes closed. Felt fresh and vigorous. Mind wandering was most disturbing at 75°rb, 30°rb, 15°rb and 0°b. At these positions there was a dull feeling in my ears. Sounds at the side seemed more piercing and usually came by pulsations. The standard of certainty was the same throughout and about the same as in previous tests. It seemed also that the process of perception and the feeling of certainty were simpler and easier at the side than either in front or in the back.

D. S. 10.00 a.m., Nov. 3, '04.

Felt fresh and bright. Had eyes closed. Sometimes the sounds seemed to come in by pulsations but I had no especial associations with them. The attention fluctuated more than in any preceding test. Often I was attending only passively to the stimulus. Frequently at the side and in front I seemed to hear the sound all the time and consequently it was difficult to stick to a standard. My visualization which is always present underwent several very noticeable changes. Beginning with 0°b in this test I visualized a line extending from the back of my head to the receiver. At 30°rb this changed and the line seemed to extend from the right ear to the receiver, until we came to 15°rf and there the line seemed to extend from the center of my forehead to the receiver.

D. S. 2.30 p. m., Jan. 28, '05.

I was in excellent condition for the test. Had eyes and mouth closed. There was a tendency toward mind wandering during the first fifteen minutes, covering the first three points. The rising of the sound to and above the thresh-

old reminded me of breathing that begins slowly and increases in rapidity and volume as the air is expelled from the lungs. The visualization had the same peculiarities as in the last record. There was a slight tendency toward rhythmic response.

D. S. 8.00 a.m., Feb. 1, '05.

Was not subject to hallucinations so much as before. Noticed very distinctly the changes in the process of visualization, mentioned before. I felt very sure of such changes and possibly they may have been a cause for the difference of threshold in different directions. I also noticed clearly a feeling of strain in my right ear when I listened 'mainly with that ear,' i. e., on the side, which I did not notice in the front or in the back. I also felt quite sure that subjectively the main difference between the side and front or back was that I felt certain more easily and quickly in the measurements on the side, as to whether I heard the sound or not; and possibly that may be one reason for the threshold being lower at the side than in front or in the back. I seemed to feel more at ease when the sound was at the side.

D. S. 8.00 a.m., Feb. 3, '05.

Was somewhat disturbed by hallucinatory sounds especially at the first few points in the beginning of the second half. Also noticed a decided feeling of strain and heaviness in both halves in the range of 45°rf to 45°rb. In the first half at 75°rf it seemed that the sound became a little higher in pitch and apparently remained there during the rest of the experiment. Sometimes it seemed that the perception of the sound was not so much the observation of a sound coming above the threshold as a gradual discrimination of it, as it became louder, from the subjective sounds which were conspicuous on account of the quietness of the room.

D. S. 8.15 a.m., Feb. 8, '05.

Began the test with a special determination to make as uniform a record as possible, that is, to make the mean variation as small as possible and have the standard of certainty the same throughout as well as I could. Again noticed very distinctly the change in visualization. Also observed that as the sound came into consciousness I heard the overtones before I heard the fundamental. First I heard the fifth, then the third, and finally the fundamental. Sounds scarcely ever came above the threshold in any other way in today's record, especially in front and on the side. Did not notice it so frequently when the sound was in the back.

N. C. 3.30 p.m., Nov. 8, '04.

The quality of the sound seemed constant, but at 0°f and 0°b the sound was hardest to hear. The places where it was hardest to hold the attention were about 45°rf and 45°rb. From 45°rb to 0°b the distance seemed twice as great as in the other directions. Fatigue was not noticed until the last quarter of the experiment.

N. C. 3.30 p.m., Nov. 9, '04.

Several images (associations) were noticed; at one time the sound seemed like the singing of a mosquito, and again like the buzzing of a track when the cars or train is a long distance away.

C. E. S. 8.00 p.m., Oct. 22, '04.

Kept mouth open because of cold. Felt more wearied just before the middle than at any other time. The first mind wandering was in the first round at 45°rb. Kept eyes closed.

The sound had entirely different qualities in different directions. In the back I do not get certain overtones. There is a point about 30°rb where there is difficulty in choosing whether to listen to the overtones or to the fundamental. The sound comes in by pulsations, about three a second, which may be heard as long as the sound lasts. The certainty is not great. I was more certain in the front quadrant than in the rear; that is, the sound seemed more distinct, rather than a change in my standard of certainty.

C. E. S. 9.00 p.m., Nov. 1, '04.

Kept mouth closed all the time. Very few trials had to be repeated. In the first half I seemed to judge by fainter standards than last time. In the last half of the latter part of the record, the standard seemed a good deal clearer than before but it was impossible for me to hear it until it had this clearness. The sound came in as beats. These beats had association with distant sounds, e. g., it was difficult for me to avoid thinking of the sound as coming from a rooster, a dog, distant singing or speaking. There were periods when I judged by these associations instead of thinking of the sound as a meaningless threshold sound. The difference in quality was not so marked this time as last. Both active and passive attention are present—the former only for a short period. Can it be that the beats are due to the 'vibration' of active effort? I located the sound in one ear all the time. The perception of direction was much less certain than for strong sounds. The sound was of higher pitch than the fundamental. Did not determine what it was—perhaps it was the fifth or octave.

H. S. B. 9.00 a.m., Nov. 8, '04.

In the experiment I heard overtones rather than the fundamental tone from 60°rf to 60°rb. In many cases I heard only a sort of fluttering noise which did not seem to have tone, but the presence of which could be distinctly detected. In nearly all cases the beginning of the sound as it came above the threshold was indefinite; but cessation of the sound was much more clearly distinguished. This gave a feeling of satisfaction in the reater certainty that the sound had really been heard and was not an illusion.

Most of the sounds were localized somewhere within my skull, some being rather high and to the front, and others being in the lower back part. About 45°rf and 45°rb there was a less degree of certainty as to when the sound began and stopped than was true of other locations.

In order to procure more specific information on some points the following list of questions was submitted to the observers after all the tests had been completed:

1. Did the sound seem different from different directions? Did you notice overtones?
2. Did you have any associations with the sound? Did the sound come by pulsations?
3. Did the receiver or source of sound seem to change in distance for different directions?
4. Was it easier to perceive the sound in some directions than in others? Which and Why?
5. Did your standard of certainty in perceiving the sound change?
6. Was there a tendency toward hallucinations and rhythmic response?
7. Did you visualize the position and direction of the sound? Was the visualization different for different directions?
8. Did you have difficulty in attending to the stimuli?
9. Did you notice any motor sensations?
10. Eyes and mouth closed or open?

The more significant observations brought out by the introspective notes and the replies to the questions may be summarized as follows:

In the beginning of the tests there was more or less uncertainty as to just when to feel sure that the sound was heard. But as the experiments progressed a fairly constant degree of confidence was adopted by each observer.

In several instances the sound was heard in pulsations, about three per second in one case, and apparently correlated with the heart beats in another.

The attention fluctuated very noticeably in most cases, which is indicated not only by these introspections but also by the actual fluctuation of sensitivity (see Figure 5). The introspections also seem to agree that it was more difficult to hold the attention during the measurements in the lateral directions than in the other directions. Some claimed to be able to pay attention better when the eyes were closed and others when the eyes were open.

With some observers visualization of the source of sound and associations with the sound were very prominent and apparently of significance to them in the process of perception. Several striking contrasts and changes accompanied the measure-

ments in the different directions. The lateral directions were characterized by entirely different visualization processes than the anterior or posterior directions.

Interpretation of the curves—The three facts which demand consideration are, that the threshold for front and back is approximately the same, that there are two types of persons in respect to the relative keenness in front and in the back, and that the threshold for the side is decidedly keener than for either front or back.

The first fact plainly shows the error of the prevailing belief and statement sometimes made that we hear sounds from the front better than from the back. If we notice the conditions present in these two directions, front and back, and the similarity in their location, we are prone to ask, why should the two be very different? Indeed the agreement in the keenness in these two directions, might have been predicted on theoretical grounds, inasmuch as these two directions are located symmetrically with respect to the ears, and the ability to localize sounds in these two regions is the same.[1] The belief that hearing from the front is finer than from behind undoubtedly rests partly upon the shape of the concha and partly upon the observation that we tend to face the source of sound when we wish to hear well. An observation made by v. Kries[2] undoubtedly finds its explanation in the above results, "———wir konnten z. B. nicht finden, dass etwa der schwächere Klang mit Vorliebe nach hinten, der stärkere nach vorn verlegt worden wäre."

In regard to the keener sensibility at the side let us first consider the introspective remarks on the characteristics, conditions, and processes of perceiving the sound in the lateral directions. An observation frequently recorded by the majority of the observers is that the sound from the lateral directions seemed nearer than from the anterior or posterior directions. But this is simply a naïve statement of what is objectively shown by the experiments, namely that since the threshold is lower at the

[1] See localization charts in Part I.

[2] Ueber das Erkennen der Schallrichtung. Zeitsch. f. Psych. u. Physiol. d. Sinn., I., 1890, 246.

side a sound is comparatively higher above the threshold, that is, relatively stronger and hence nearer.

A still more significant fact is the repeated statement in the introspective notes that it is 'easier to perceive the sound' when it comes from a lateral direction. The same observation was stated in various ways. 'I feel more at ease,' or 'more confident' when the sound is on the side. It is 'more piercing' on the side. 'Hard to pay attention,' or 'was not so sure in response when the sound was behind me.' 'Hardest to hear in front and back.'

Then there were striking changes in the forms of visualization in symmetrical places in the front and rear quadrants. For example, see above the note of D. S., Nov. 3. The shifting of the visualization process and the changes in the facility of the attention seem to have accompanied each other. The question arises, were there similar changes in the process of perception in passing from anterior or posterior directions to lateral directions? One observer mentioned (see above H. S. B., Nov. 8), that the overtones in the directions between 60°rf and 60°rb were much more prominent. The fact that the source of sound at the side is more favorably located with reference to the ear on that side no doubt accounts for the lower lateral threshold and for the qualitative changes mentioned by the observers. Do we not actually take advantage unconsciously of the keener sensibility on the side? In an audience one may frequently observe people trying to hear the speaker better by turning the side of the head toward him.

The individual difference among the observers in regard to the acuity for front and back divides them into two distinct types: (a) those who have greater keenness in the front and (b) those who have greater keenness in the back. The discovery of this difference in the first few tests led to the two extended series of records shown in Figures 1 and 2. The object was to determine whether this distinction would be maintained permanently or whether it was only an individual deviation which would be counterbalanced by additional tests. But the results show that the two types were clearly maintained. The difference for front and back for these two types is .5 for

N. C., Figure 1, in favor of front, and 1.3 for D. S., Figure 2, in favor of back. This difference is even more conspicuous in the monaural records of the same observers, Figure 4, being 2.7 for N. C. in favor of front and 1.3 for D. S. in favor of back. The amount of difference is too great to be accidental, as in each case except the first it is more than one unit of measure on the audiometer.[1]

What accounts for these two types? One factor that might be suggested is undoubtedly the differences of the anatomical structures of the ears, especially the pinnae. A slight difference in the course of the meatus and in the adjoining structure may also possibly render the perception of sound easier from the rear in one individual or easier from the front in another individual.

Another reason for the keenness ofdetecting sounds from the rear may be sought in the phylogenetic development of the race. The ear rather than the eye has been the means of detecting sources of danger in the rear, and consequently the auditory habits have adjusted themselves to serve this purpose.

Only one series of directions, the horizontal plane through the aural axis, was tested in this series, but the results may safely be generalized and applied to the vertical planes as well, on the ground that there is uniformity in the localization records for the horizontal and vertical planes and uniformity in general conditions for these two sets of planes. If one of the composite curves, Figure 3, were revolved on the point 90°r as the center in such a way that its axis would coincide with the aural axis, it would generate a saucer-like surface which would probably represent the keenness of sensitivity for all possible directions of the right hemisphere and analogously also for the left hemisphere.

Monaural Threshold.

In order to measure the monaural threshold and to compare it with the binaural, six monaural records were obtained from N. C. and D. S. The conditions and method of measurement

[1] One unit can be perceived as an increment with a fair degree of certainty.

were exactly the same as in the preceding experiments, excepting that the left ear was heavily bandaged.

Table II gives the individual records. In Figure 4 the average monaural and binaural curves of the two observers are presented together for direct comparison. The binaural curves are taken from Figure 3.

TABLE II.

N. C.

0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
13.8	12.5	11.9	12.0	10.8	10.3	10.0	10.8	11.7	12.2	14.0	15.8	16.9
13.2	11.6	11.3	10.7	10.3	9.2	9.2	9.0	9.8	10.8	11.7	12.0	15.0
12.2	11.9	10.6	10.5	9.7	8.7	8.6	9.3	9.5	11.0	12.7	14.4	15.6
13.1	12.0	11.3	11.1	10.3	9.4	9.3	9.7	10.3	11.3	12.8	14.1	15.8

D. S.

16.5	14.9	13.7	13.2	11.4	10.8	10.7	10.8	11.1	11.5	12.5	13.2	14.6
16.8	16.8	15.3	13.9	12.5	11.1	11.3	11.7	12.4	12.1	12.5	13.8	14.8
15.5	15.7	14.5	12.3	11.1	10.8	11.6	11.9	12.4	12.8	13.5	14.2	15.5
16.3	15.8	14.5	13.1	11.7	10.9	11.2	11.5	12.0	12.1	12.8	13.7	15.0

The mean variation for these records is about the same as for the binaural measurements, about .6.

In general the results present no new features: front and back are approximately on the same level; both observers maintain their respective types, N. C. with lower threshold in front and D. S. in back; the side is considerably keener than either front or back.

The monaural threshold in the lateral directions is practically the same as the binaural, but in the anterior and posterior directions the monaural threshold is a little higher. The ratios of side to front and back in the monaural tests are .64 (N. C.) and .69 (D. S.) while in the binaural tests they are .73 (N. C.) and .72 (D. S.). Evidently the exclusion of the left ear does not affect the lateral thresholds on the right side, but the anterior and posterior are slightly higher.

It is evident then from the close agreement of the monaural with the binaural thresholds that the acuity of hearing is not dependent upon any coöperation of the two ears so far as the hemisphere on the side of the active ear is concerned. On the side of the hearing ear we hear as well with one ear as with two. But in respect to discriminative processes, such as are involved in the auditory perception of direction, the combined action of two ears is decidedly better than monaural perception. This will be considered later in greater detail.

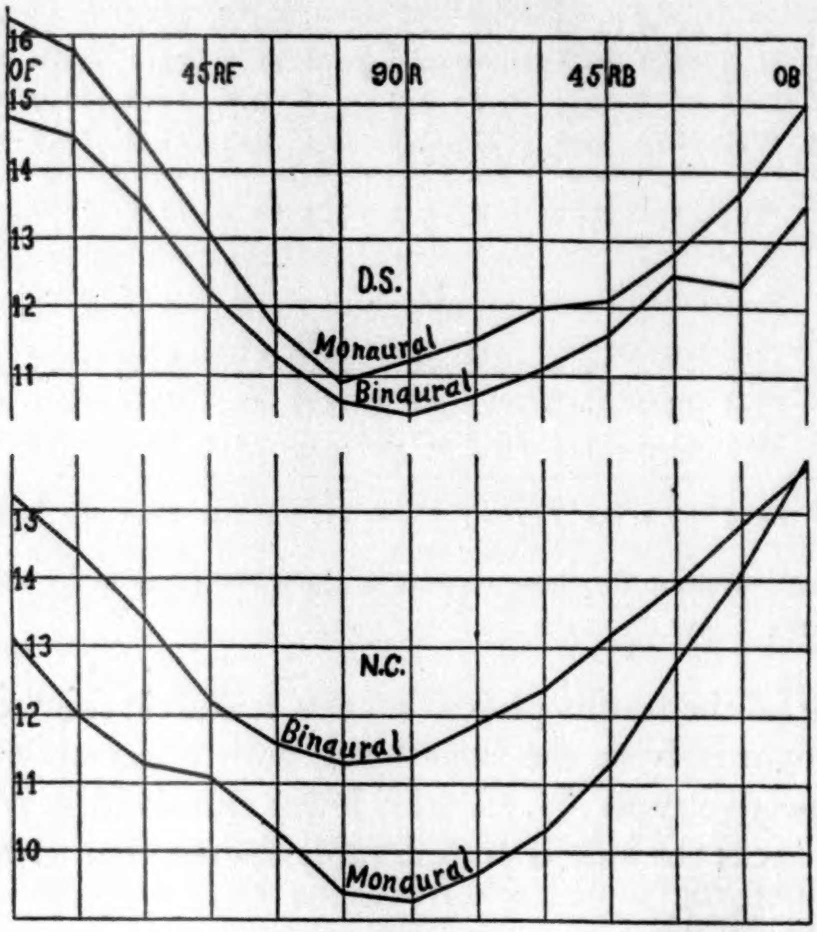


FIG. 4

The Bearing of the Threshold Experiments upon the Localization of Sound.

The localization of sound has been shown to be much less accurate on the side than in front and in the back. The threshold of hearing, on the other hand, is considerably lower, that is, keener on the side than in front and in the back. The curves of localization and of the threshold measurements take

directly opposite courses. Hence, acuity and localization do not run parallel and are not direct functions of each other, nor are the two curves reciprocals of each other. The ratio of the thresholds of the side to front and back is, as shown above, 3 to 4, while the same ratio for the localization is about 4 to 1.

Localization depends upon discrimination, while hearing ability or acuity is an expression of sensitivity. Localization is primarily a binaural process while hearing ability is mainly a monaural matter. Localization is most accurate in anterior and posterior directions where the coöperation of the two ears is at a maximum; it is least accurate in the lateral directions where the coöperation is at its minimum. If one ear be excluded the localization will be greatly impaired in those directions in which the coördinating activity is most important,[1] but on the other hand, the hearing ability will remain practically unaffected. Localization involves processes of discrimination and is primarily binaural, just as the perception of visual space and volume is dependent largely upon binocular vision, whereas the sensibility to light is probably as acute in one eye as in two. Of course, monaural localization is possible just as monocular space perception is possible, but it is not as accurate and reliable.

Sensibility and discrimination do not necessarily depend upon each other nor vary together. A person with a low threshold will not necessarily localize more accurately than one with a high threshold.

The significance, then, of the present threshold measurements for localization is this: They demonstrate precisely what had been merely a supposition, namely, that a sound on the side in the region of the aural axis does seem stronger and nearer than in front or in the back, and of this difference we have obtained a quantitative measurement. A sound seems stronger when near the aural axis than when farther away and these variations in intensity are potent factors in rendering our perception of direction accurate.[2]

[1] See the last series in this report on monaural localization.

[2] These results corroborate the discussion in Part I, 17 ff. Further, the equal sensitivity to sound in front and in the back contradicts the popular assumption that we tend to place weak sounds toward the rear and loud sounds toward the front.

General Observations.

There are two main individual differences in auditory sensibility. First, in respect to the relative acuity in front and in the back, individuals are divided into two classes. Second, the threshold is much higher for some than for others. Compare for example the records of C. E. S. and E. G. Q. in Table I. The threshold of the latter is about twenty units higher. This may be due to differences in mental attitude during the tests, in the standard of certainty in responding, or in the anatomical structure of the sense organs.

The auditory acuity is greater on some days than on others; compare, for example, records 6 and 10 in Figure 1. These daily fluctuations find their explanation in the variation of subjective conditions and perhaps also to a slight extent in the unavoidable objective variations.

The records on successive days[1] show some improvement. There is a tendency for successive curves to be progressively lower in the scale. This improvement is probably not in the sensibility of the ears but rather in increased familiarity with the sound and consequently increased power to direct and control the attention in the experiments, and possibly also in the adoption of a fairly constant standard of certainty. The uncertainty as to whether the sound was heard or not, disappeared after the first record. The improvement is thus due to practice, and to familiarity with the situation of the experiment, rather than to any increase in the actual sensitivity of the sense organ.

The recent demonstrations of fluctuations and periods in mental acuity and application[2] show that these factors enter into the results of all forms of psychological experiments and particularly into continuous work such as was required in these threshold measurements. Each record represents practically continuous activity for approximately forty-five minutes. The

[1] The order of the records is indicated by the small numbers at the left end of the curves.

[2] Kraepelin, "Die Arbeitscurve." Phil. Studien, XIX, 1902, 459–507. Seashore and Kent, "Periodicity and Progressive Change in Continuous Mental Work." Univ. of Iowa Studies in Psych., IV, 1905, 47–101.

only breaks occurred when the observer had to turn from one direction to the next. Although no special effort was made to follow any set time, the stimuli and the reactions followed quite uniformly at intervals of from 5 to 8 seconds.

To show the fluctuations and to bring out the types of periods discovered by Seashore and Kent the following two curves, Figure 5, are presented from the records of N. C. and D. S. Each curve represents one complete record. The points in the zigzags are individual reactions. The horizontal bars represent the averages of groups of the ten determinations which were made at a time in a given direction. The breaks in the curve between the groups of tens represent the change in position on the part of the observer from one direction to the next. Each curve is composed of two parts, being the two parts of the double fatigue order from 0°f to 0°b and then from 0°b to 0°f. The continuous line in each curve is the composite of the ten records obtained from the two respective observers, and is to be regarded as the basis upon which the zigzag curve is drawn, the latter being one of the ten records in detail. The composite curve is used as the basis because all the detailed fluctuations are obliterated in it and it thus affords the only standard by which to detect the individual deflections of the separate records. It must be borne in mind that the wave-like form of the average lines is the typical variation in the threshold for the different directions as pointed out above, and not the fluctuation in mental work. Regarding the continuous lines as the base lines we notice that each record shows two complete waves of fluctuation. For example, the upper curve begins above the base line, gradually falls below it, rises above again at 0°b, then falls below reaching its lowest point at 45°rf when it rises to the level of the base line at 0°b. These large waves correspond to the small hour waves of Seashore and Kent.[1] The zigzags of the separate reactions probably coincide with the crests of the "second-waves" six to eight seconds long, which was approximately the period of one reaction. Then in the groups of tens there are frequently smaller groups of two to seven reactions

[1] Op. cit. p. 55.

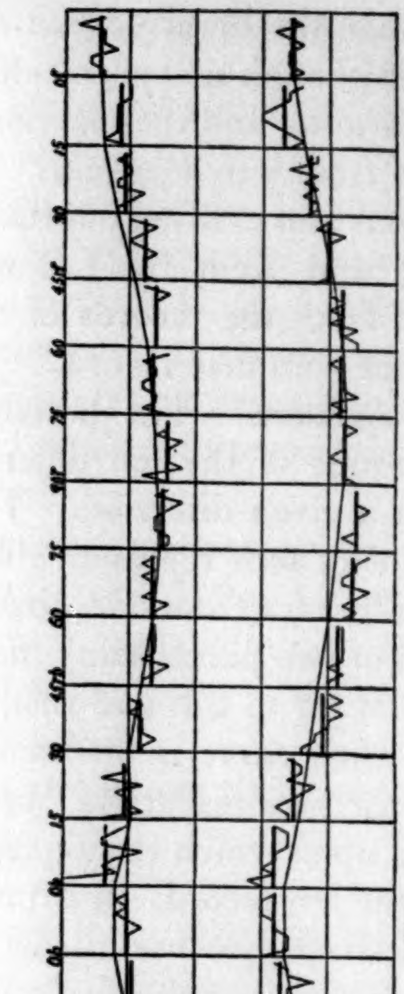


FIG. 5

which are either all above or below the horizontal bars. These correspond to the 'minutes waves.'

The periods of drowsiness and difficulty in paying attention which were experienced by the observers during the experiments probably coincide with the larger fluctuations. From the point of view of the present tests these fluctuations in mental work are disturbances and account for the deviations of the individual curves from the averages. But their effect is not serious because it is neutralized in the composite curves.

SERIES II.

Discrimination of Intensity and Pitch in Different Directions.

The differences in threshold in different directions as found in the last series of experiments suggested the problem as to whether the keenness of discrimination between intensities or pitches or other qualities of sound would also vary with different directions. The aim of this series of experiments was to determine whether the discrimination varies in some regular manner for the series of directions in which the hearing ability measurements had been made.

Discrimination for Intensities.

The apparatus, accessories, source of sound, location and distribution of the apparatus, were exactly the same as in the threshold experiments. The only difference was in the method.

Before the regular tests were begun the threshold of hearing was found for each observer and then a sound ten units stronger (on the audiometer scale) was chosen as the standard intensity. This sound was strong enough to be easily heard.

In making the measurements the experimenter gave the standard sound for one second and then after an intermission of one second, sounded for one second either the standard or the sound one unit stronger. The problem for the observer was to deter-

mine whether the second sound was the same or a stronger. If he judged it to be the same he responded with one tap on the strap key; if stronger, with two taps. The increment of one unit was found to be large enough to be perceived as an increment and yet not too large to make the observer absolutely certain. Ordinarily about 75% of the judgments would be correct.

Twenty determinations were made in one sitting, in the double fatigue order, for each one of the thirteen directions on the right side. This constituted one record.

The tests were made on three observers (men), C. E. S., E. A. R. and D. S. For the first observer, the standard intensity on the audiometer scale was thirty, for the other two, twenty-five. Two of the observers made three records each, and the third made two records. Thus 160 determinations were made for each direction, or a total of 2,080.

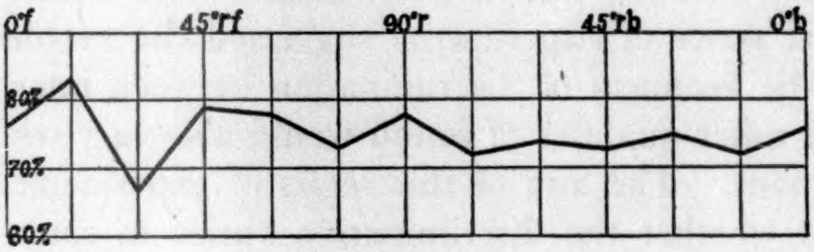


FIG. 6

The results of these measurements are represented in the curve of Figure 6, which is the composite of all the records. The points in the curve were determined by calculating the percentage of correct responses.

It is evident from a glance at the curve that the discrimination between intensities is the same in all directions, each showing about 75% correct judgments. If a still larger number of records had been obtained the curve would probably have become almost a straight line. The results, although largely negative, corroborate the suggestion made above that keen sensitivity is not necessarily accompanied by accurate discrimination. It also demonstrates that the greater accuracy in localization in front and in the back than at the side, is not due to greater accuracy in discriminative ability in those directions.

Discrimination between Pitches.

The apparatus employed for the purpose of determining the discriminative ability between pitches was a set of tuning forks customarily used in the Iowa laboratory. It is a set of eleven forks of uniform size and shape—11.5 cm. long—tuned to produce successive increments above the pitch of international A, 435 v.d. as follows: ½, 1, 2, 3, 5, 8, 12, 17, 23, and 30 v.d.

The sound of the forks had to be augmented by a resonator because the source of stimulation had to be at a distance of one meter from the observer's head. An upright glass tube served as resonator. The experiments were made in the quiet room of the laboratory.

In making the tests the standard fork—435 v.d.—and one of the differential forks were sounded in rapid succession by striking them uniformly and holding them over the resonator. The observer was allowed a choice of only two answers, namely whether the second tone was higher or lower than the first. The approximate discriminative sensibility of each observer was found by a few preliminary trials. Ten trials were made at one time in one direction. If less than 70% were correct a larger increment was taken, if more than 80% were correct the increment was decreased. Twenty trials for each of the thirteen directions were made in one sitting in the double fatigue order.

The increment required for 75% correct judgments was calculated from Fullerton and Cattell's table.[1] The narrow limits of 70% and 80% were chosen as the data for the calculation in order to make the calculated increment reliable. An increment calculated on the basis of a record in which 60% or 90% are correct would not be empirically valid, especially when the increments are so small as in the present tests, being within .5 and 2. v.d.

Twelve records were obtained—from C. E. S. one, from E. A. J. six, and from D. S. five. These gave 240 trials for each direction or a total of 3,120.

[1] "On the Perception of Small Differences," Univ. Penn. Phil. Series, No. 2.

The variation of the observers from one another was within a narrow range (.8 to 2.1 v.d.) so that it is not necessary to present the records individually. They are summarized in the composite curve in Figure 7. The abscissae represent the directions and the ordinates represent the amount of difference in pitch, measured by vibrations.

The interesting feature of the curve is that the discrimination is considerably poorer on the side than either in front or in the back, the front is slightly better than the back. Although the difference in discrimination between 0°r and 0°f is only .5 of a vibration, it is relatively large, amounting to over 40%. All the

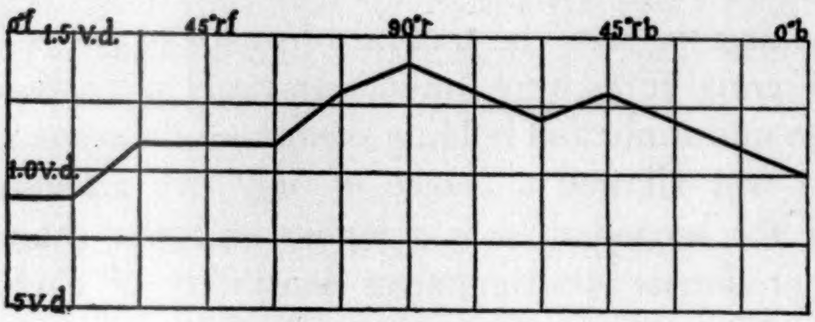


FIG. 7.

records agree in this respect. As an explanation of the poorer discrimination at the side it might be suggested that the ear probably catches more overtones when the source of sound is on the side. This may be confusing and consequently cause poorer discrimination.

The localization of sound is more accurate in front and in the back than on the side. Likewise the discrimination between the qualitative characteristics of tones, according to the pitch tests, seems to be better in front and in the back than on the side. It appears reasonable to infer that the greater accuracy of localization in front and in the back may be due in part to the greater accuracy in discriminating between the qualitative characteristics of sounds in those directions.

SERIES III.

Quality[1] of Sound in Localization.

It became evident from the experiments on localization described in Part I that the character of the sound is of considerable importance as a datum for discerning direction. As demonstrated by some special tests[2] the sound seemed clearer, richer, and fuller in some directions than in others, and these qualitative differences are constantly made use of unconsciously to differentiate the directions of sound.

The problem then is, can a sound whose complexity and qualitative characteristics are reduced to a minimum be localized as accurately as a richer and more complex sound? To what extent does localization depend upon these characteristics?

It is quite evident that this problem can be approached by choosing a variety of stimuli differing as widely as possible in their nature and complexity, and comparing the accuracy with which they can be localized. The following stimuli were selected.

a. The singing flame. This was produced in the usual manner. Ordinary illuminating gas, containing about 60% hydrogen, was used. The resonator tube (glass) was 25.5 cm. long and 1 cm. in diameter. The height of the flame was 8 mm., and the pitch of the sound about 730 v.d.

It was necessary in the experiments to interrupt the sound when moving from one direction to another. This was accomplished by shutting off the supply of gas for an interval long enough to stop the tone, after which the flame would again rise to its usual height and produce the sound. Although the singing flame is not strictly pure it was considerably purer than any one of the other sounds used.

b. The Galton whistle. Three pitches were used, 10,000, 20,000 and 30,000 vibrations. The air current was supplied by constant pressure tanks.

[1] The term quality is here used as physicists use it, in the sense of timbre.
[2] Part I, 18 ff.

c. The voice. The word 'now' was pronounced with the intensity and clearness of ordinary speech.

d. An electric hammer. A small wooden hammer, 2.5 cm. long and 2 cm. in diameter, was struck against a block of wood 5.2 cm. long and 2.7 cm. in diameter. The hammer and block were mounted on an ordinary electric bell, the block was put in place of the gong and the hammer fastened to the armature. The automatic make and break was removed so that the strokes of the hammer could be controlled as the experiment required. The use of magnets in the circuit of a constant current by which the hammer was struck made it possible to produce strokes of uniform amplitude.

e. A clapper. A small board, 12.5 cm. x 6 cm. x 5 cm., and another piece of wood, 15 cm. x 2 cm. x 1.5 cm., were fastened together at one end by a hinge and a spring. By opening the clapper and releasing one wing, it would strike against the other wing producing a clashing noise. The clapper was manipulated by a string.

f. A whiff of air. The air supplied by pressure tanks was conducted through a rubber tube terminating in a small glass jet. The whiff was produced by opening and closing the rubber tube which furnished the air current.

These various devices for producing stimuli were attached to the arms of the sound perimeter. In case of the voice the experimenter stood in such a position that his mouth took the place of the mechanical devices. The method followed in the localization experiments of Part I was also employed here and the results were treated in the same way.

Three representative directions were chosen, 0°f, 45°rf, and 90°r, in the horizontal plane through the aural axis. It was not necessary to test more than these three directions for the purpose of comparing the different stimuli, because they represent the typical extremes in localization, and as demonstrated by the earlier experiments the curve of the right front quadrant is symmetrical with the curve of the right back quadrant.

The results are summarized in Tables III–V. The observers and the number of trials are mentioned in connection with each table.

TABLE III.

The Singing Flame.

Observers.	0°f	45°rf	90°r
N. C.	10.6	37.4	34.5
N. B.	6.4	15.8	29.8
H. S. B.	9.5	30.8	42.7
E. A. J.	14.9	30.0	*
Averages	10.3	28.5	35.7

* The discrimination at this point seemed to be too crude to make a satisfactory measurement.

TABLE IV.

The Galton Whistle.

Observers.	Pitch	0°f	45°rf	90°r
C. E. S.	10,000	5.4	*	46.1
	20,000	3.1		29.0
	30,000	6.2		54.6
D. S.	10,000	2.9	9.2	43.5
	20,000	2.5	5.8	34.8
	30,000	2.6	5.1	25.0
R. W. S.	10,000	2.1	12.5	28.5
	20,000	1.7	7.5	18.7
	30,000	1.7	8.0	23.1
A. K.	10,000	6.2	6.5	50.0
	20,000	8.8	13.1	35.9
	30,000	5.0	12.2	40.8
Averages	10,000	4.1	9.4	42.0
	20,000	4.0	8.8	29.8
	30,000	3.4	8.4	35.9

* No tests were made.

TABLE V.

Noises and the Voice.

	0°f				45°rf				90°r			
Observers.	Hammer	Clapper	Whiff	Voice	Hammer	Clapper	Whiff	Voice	Hammer	Clapper	Whiff	Voice
C. E. S.	1.8	1.5	2.6	1.5	4.4	3.4	7.2	2.6	10.6	37.7	25.6	9.5
D. S.	1.6	1.5	1.5	1.5	4.0	2.2	3.1	2.2	26.3	14.4	7.3	3.6
A. K.	2.4	1.5	1.6	1.5	3.6	3.1	3.6	2.4	10.5	12.8	11.6	9.2
C. L. V.	2.6	2.2	2.4	1.5	4.4	2.5	8.2	2.2	29.0	29.0	25.6	11.6
Averages	2.1	1.7	2.0	1.5	4.1	2.8	5.5	2.3	19.1	23.5	17.5	8.5

With each stimulus 50 determinations were obtained from each observer for each direction, total 2,400.

The figures in the table represent in degrees the just perceptible difference between directions. One hundred determinations were made with each observer for each direction, in all 1200.

In order to compare more directly the accuracy of localization of the different stimuli the results are presented graphically in the curves of Figure 8. The radii represent the directions and the arcs represent degrees of just perceptible difference between directions. The curve shows only the results obtained with the 30,000 pitch for the Galton whistle.

The main results may be summarized as follows: The singing flame, which is an approximately pure tone, is localized very poorly. The high tones of the Galton whistle which are

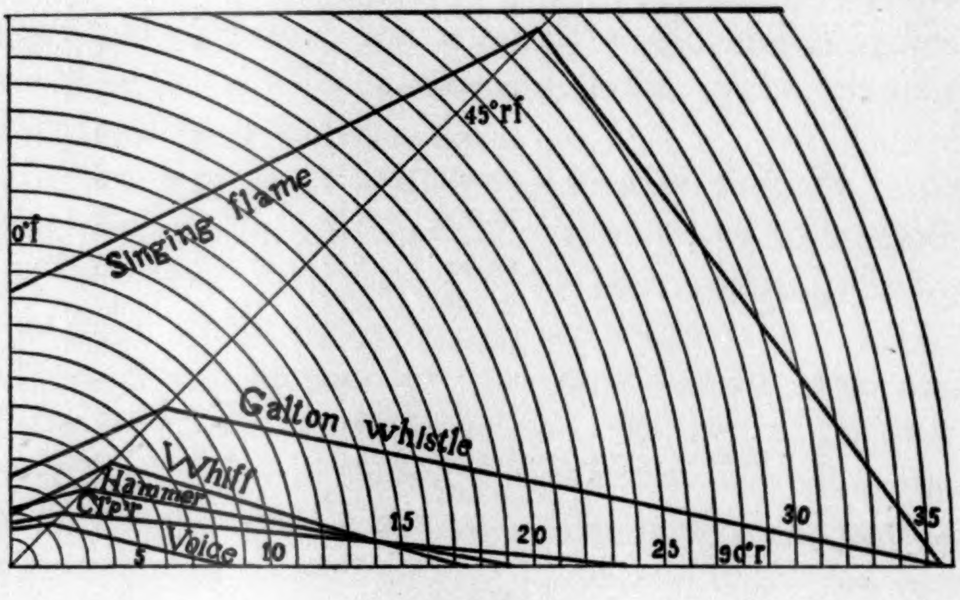


FIG. 8

quite free from overtones are also localized rather inaccurately. The noises are localized more accurately and the voice most accurately. The voice is probably not more complex than some of the noises but its overtones are more regular and continuous and within the customary range of hearing. In general, then the richer and more complex a sound the more accurately can it be localized. The complex sounds are the most frequent sounds in our experience, and the two factors, frequency and complexity, have coöperated in our learning to localize them

most accurately. These experiments corroborate the results obtained by Professor Angell in so far as a comparison can be made.[1]

The problem of quality was approached in still another way. An effort was made to obtain a tone that might be purer than the singing flame. So Helmholtz's method of using a fork and a resonator was adopted.[2] A fork of 250 v.d. and a Helmholtz resonator were mounted on a convenient wooden frame, which was attached to one of the arms of the sound perimeter. All points of contact were supplied with soft rubber in order to avoid any resonance from the frame. To eliminate the buzz caused by the interrupting contact, the current was interrupted by another fork of the same vibration frequency, located in a distant room. A double circuit was arranged so that the driving fork would be running constantly and yet at any moment the current could also be sent through the magnets of the stimulus fork for any desirable length of time. The tone thus produced was a close approximation to a pure tone, at least none of the other resonators in the Helmholtz series brought out any tones.

For the purpose of comparing the localization of this tone with a complex tone the sound from an ordinary telephone receiver was used. The current for this was interrupted by a 100 v.d. fork. The rest of the apparatus consisted of the sound perimeter by means of which the stimuli were controlled.

The method of experimentation may be described as a type of the method of production. Twenty-four directions, 15° apart, in the horizontal plane through the aural axis were used. The observers were told before the experiments that the stimuli would come from any of these points. The stimulus was given from one of the points and the observer then answered by means of the adopted nomenclature with which each observer had been made familiar before the experiments.[3] The experiments with the fork and the telephone were made simultaneously, that is,

[1] Angell, "A Preliminary Study of the Significance of Partial Tones in the Localization of Sound." Psych. Rev., X, 1900, 1–14.

[2] Helmholtz, "Sensations of Tone." Trans. by Ellis, third ed. 120, 339.

[3] See Figure 2 in Part I.

an equal number of determinations were made with both stimuli in each sitting. This was done in order to make the effect of practice and other conditions uniform for both. Thirty-six determinations were obtained for each of the twenty-four directions from six observers, four men and two women, in all 864 trials.

The results are given in Table VI.

TABLE VI.

Observers.	Number correct	Misplaced 15°	Confusions	Right-left misplaced	Others
		Telephone.			
D. S.	98	43	2		1
F. O. S.	74	56	5		9
R. W. S.	99	41	2		2
F. V.	78	47	8	1	10
E. L.	69	65	1	1	8
C. P.	97	43	1		3
Totals	515	295	19	2	33
Percent	59.6	34.1	2.2	.2	3.9
		Tuning Fork.			
D. S.	22	31	27	9	55
F. O. S	12	39	26	14	53
R. W. S.	19	33	22	13	57
F. V.	25	30	16	12	61
E. L.	33	41	13	13	44
C. P.	70	50		6	18
Totals	181	224	104	67	288
Percent	20.9	25.9	12.0	7.8	33.3

The significance of these figures is clear. They serve to emphasize the importance of quality and complexity of sound as data in the perception of direction. The complex sounds of the telephone are localized correctly 59.6% times as compared with 20.9% for the fork. But the difference is even greater when we compare the other columns of the table. More telephone sounds are localized approximately correctly, i. e., misplaced only 15°, so that if we add to correct localizations those

approximately correct we get for the telephone 93.7% while for the fork only 46.8%.

The column headed 'confusions' refers to those sounds which were located in a symmetrical position, or within 15° of the symmetrical position, in the quadrant on the same side. For example, if a sound at 60°rf was located at 60°rb, or within 15° of that, it is counted as a confusion.[1] These confusions are much more frequent with the pure tone than with the complex tone, the telephone 2.2% and fork 12.0%. The column of right-left misplacements refers to confusions, not necessarily symmetrical, of the right and left sides, and these also occurred oftener with the pure tone, 7.8%, than with the complex tone, .2%. The last column contains all those which are not accounted for under the preceding headings. These experiments, therefore, fully corroborate the conclusions of the preceding experiments, that a complex sound is much more accurately localized than a relatively pure tone.

It has been suggested that a pure tone can not be localized. While this cannot be positively contradicted on the basis of the preceding experiments, it nevertheless seems quite probable that an absolutely pure tone can be localized with some degree of accuracy. Even if the qualitative data in the perception of direction were completely eliminated in the case of a pure tone, the binaural ratio of intensities would still remain, by means of which some clue to direction would be indicated.

SERIES IV.

Misplacements in Localization.

Closer scrutiny of the errors and misplacements of the telephone sound reveals interesting tendencies to misplacement toward certain directions. The localizations of the telephone sound are therefore presented in detail in Table VII, in which the objective positions of the stimuli, the number of correct localizations, and the number misplaced (the coefficients) to

[1] Cf. Pierce, "Studies in Space Perception," p. 70.

the designated positions (in parentheses) are given. The four cardinal points, 0°f, 90°r, 0°b, and 90°l, are not included in the summary for reasons given below. The localizations of the telephone sound, rather than of the fork, were used because they are more accurate and bring out the misplacements more consistently and more rigidly.

TABLE VII.

Standard directions	Number correct	Misplacements.					
0°f	32	3(15°rf)	1(15°lf)				
15°rf	17	13(30°rf)	(0°f)	1(0°b)	3(45°rf)		
30°rf	21	12(45°rf)	1(15°rf)	2(60°rf)			55 backward
45°rf	26	5(60°rf)	3(30°rf)	2(75°rf)			15 forward
60°rf	23	10(75°rf)	2(45°rf)	1(90°r)			3 confusion
75°rf	19	7(90°r)	7(60°rf)	2(75°rb)	1(60°rb)		
90°r	19	6(75°rf)	6(75°rb)	4(60°rf)	1(45°rf)		
75₀rb	12	15(90°r)	3(60°rb)	3(75°rf)	1(45°rf)	2(45°rb)	
60°rb	14	9(75°rb)	12(45°rb)	1(90°r)			45 forward
45°rb	22	5(60°rb)	6(30°rb)	1(75°rb)	1(0°b)	1(15°rb)	39 backward
30°rb	21	8(45°rb)	5(15°rb)	1(75°rb)	1(45°lb)		4 confusion
15°rb	21	4(30°rb)	9(0°b)	1(0°f)	1(45°rb)		
0°b	33	1(0°f)	1(15°lf)	1(15°lb			
15°lb	24	3(30°lb)	5(0°b)	2(0°f)	1(15°lf)	1(15°rb)	
30°lb	23	7(45°lb)	4(15°lb)	1(75°lf)	1(0°b)		
45°lb	23	3(60°lb)	8(30°lb)	2(75°lb)			42 forward
60°lb	13	8(75°lb)	10(45°lb)	4(90°l)	1(75°lf)		30 backward
75°lb	16	14(90°l)	2(60°lb)	2(75°lf)	1(60°lf)	1(60°rb)	6 confusion
90°l	24	7(75°lf)	4(75°lb)	1(60°lf)			
75°lf	20	9(90°l)	6(60°lf)	1(75°lb)			
60°lf	20	8(75°lf)	7(45°lf)	1(90°l)			
45°lf	26	7(60°lf)	2(30°lf)	1(75°lf)			45 backward
30°lf	24	9(45°lf)	3(15°lf)				22 forward
15°lf	22	10(30°lf)	4(0°f)				1 confusion

It is evident that the majority of the misplacements in the anterior quadrants are backward and in the posterior quadrants forward. In other words, there is a strong tendency to shift sounds away from 0°f and 0°b around toward the aural axis. In the front quadrants the sounds are shifted rearward, 100 backward and 37 forward; and in the rear quadrants they are shifted forward, 87 forward and 69 backward.

A possible explanation was thought to lie in the suggested overestimation of angular differences between directions, for the reason that in the nomenclature here used the two most prominent directions are 0°f and 0°b, and all other directions are designated as so many degrees to the right or to the left of these two reference points. For example, if a sound was given at 30°rf the question that at once arose in the observer's mind was, how many degrees is it from 0°f? And similarly in the rear quadrants 0°b served as the point of reference. Since the tendency was to shift the sounds away from 0°f and 0°b it seemed possible that it might be due to an overestimation of angular differences.

Another observation which seems to support this supposition is the fact that two of the observers, E. L. and C. P., show a decided tendency in the rear quadrants to shift the sounds rearward instead of forward as in the case of the other observers. This accounts for the fact that the predominance of the forward shiftings in the rear quadrants is not as great as the rearward shiftings in the front quadrants. In looking for an explanation of these two exceptions it was noticed that these two observers had a natural tendency to use 90°r and 90°l as the two points of reference for the localizations in the rear quadrants instead of using 0°b as the other observers did, while in the front quadrants they used 0°f, just as the others did. If, for example, a sound appeared to be at 60°rb they would almost invariably say that it was 30° back of 90°r. Since this method of designating directions was not contrary to any specific conditions of the tests the experimenter did not object to it. Now the thing to be noted is that if 90°r and 90°l were their points of reference for the rear quadrants the overestimation of the angular difference between these points and any given direction in these quadrants would tend to shift the sounds backward.

In order to determine this more specifically the tests for the rear quadrants were repeated with these two observers. They were told that they should always locate the sounds with reference to 0°b and not with reference to 90°r and 90°l. The number of determinations is the same as in the original tests so that the results are directly comparable.

E. L. made 8 forward and 10 backward misplacements in the original tests. The suggestion to change the method of designating the directions seems to have been successful at least to the extent of having approximately the same number of misplacements in each direction. C. P. made 3 forward and 25 backward misplacements as compared with 7 forward and 22 backward in the original tests. Here there is no essential change in the results. The suggestion to change the method does not seem to have been sufficient to overcome the more natural method of designation.

Some special experiments were planned to test in a more crucial way the possible overestimation or underestimation of angular differences between directions. Several small angles were chosen in two regions, in front and on the side, as follows: 3°, 5°, 10° and 15° in front with 0°f as the standard; and 10°, 15°, 20° and 30° on the side with 90°r as the standard. These experiments were made with the sound perimeter, using the telephone sound as stimulus.

The method of average error was followed. For instance, to obtain estimates of the angle 5° in the anterior region, the experimenter gave the stimulus at 0°f, then moved the receiver 5° either to the right or to the left and there gave the sound again in the same way, and then moved the receiver back to the original position. The observer was then told to open his eyes and by means of a pointer push the receiver from 0°f to the point where he thought the second sound was. Then the reading in degrees was taken and recorded. The observers were told that the angles varied in size but did not know the size of the angles, nor that a definite series of angles had been chosen, nor were they told of the nature of their judgments until the tests were finished.

Table VIII gives the results. Each observer gave twenty estimates of each angle, total 440. The figures in the table are percentages of overestimation or underestimation, plus meaning overestimation, and minus underestimation.

The results are decisive. The angles in front are considerably overestimated and those on the side are decidedly underestimated. There are only two exceptions and these are small.

TABLE VIII.

	0°f				90°r			
Angles	3°	5°	10°	15°	10°	15°	20°	30°
G. K.	60.	54	33.	32.2	..	-22.	-22.	-22.5
P. K.	26.	20.5	4.	-1.5	-45.	-38.5	-36.5	-11.5
P. L.	..	55.	16.	4.8	5.5	-16.6	-25.7	-34.3
Averages	43.	43.2	17.7	11.8	-19.8	-25.7	-28.1	-22.8

The angles chosen for the region in front are smaller than those used on the side for the reason that our discrimination between directions is there much finer.[1] The ratio is about 1 to 4, so that a difference of 3° in front is as easily perceived as 10° or 12° at the side. Two places in the table, one under 3° and the other under 10°, are blank because these differences were too small for those observers to detect with certainty and so no estimates were obtained.

It seems quite probable that this difference in discriminative ability in these two regions accounts for the overestimation and underestimation. To put it boldly, we overestimate in front because we can discriminate between smaller angular differences in direction than we think we can, and we underestimate on the side because we do not discriminate between as small angular differences in direction as we naïvely suppose. It frequently occurred during the course of these tests that in testing, for example, the angle 10° in the lateral region, the observers would say that the difference is very small and then they would move the receiver perhaps 7° or 8°, whereas in testing the same angle in the frontal region the difference would seem considerably greater and consequently the receiver would be moved 13° or 14°. This illusion may appropriately be called the 'auditory small angle illusion.' The overestimation and the underestimation seem to be primarily central processes and not peripheral.

[1] See Part I, Figure 3, p. 10.

SERIES V.

Monaural Localization of Sound.

Monaural localization presents various problems whose solution would throw considerable light upon binaural localization. In the experiments on the monaural localization three classes of observers were employed: (a) Persons in whom monaural conditions were produced artificially, (b) one person whose left ear was partly defective, and (c) two persons who had been completely deaf in one ear for many years.

Artificial Monaural Conditions.

The accuracy of discrimination between directions was investigated. The directions, 0°f, 45°rf, 90°r, 45°rb and 0°b in the horizontal plane through the aural axis served as standards. The modified form of the method of right and wrong cases which has been adopted for all the discrimination tests throughout this investigation was also followed here. The telephone was used as stimulus.

The tests were first made upon two observers in whom monaural conditions were produced artificially by closing the left ear by means of inserting a finger firmly in the meatus of the ear.[1] The intensity of the stimulus was so adjusted that when both ears were thus closed the sound could not be heard at all. The figures in Table IX represent in degrees the smallest differences that could be perceived between directions.

Table IX.

	0°f	45°rf	90°r	45°rb	0°b
C. E. S.	5.8	14.3	17.4	12.0	9.5
D. S.	6.6	7.3	14.5	7.3	11.6
Average	6.2	10.8	16.0	9.7	10.6

[1] The main objection to this method of closing the ear is that the circulation of the blood becomes quite noticeable in the closed ear, and consequently disturbs the attention. But this method was preferred to bandaging because it was almost impossible to bandage one ear sufficiently to exclude the sound.

Twenty-five determinations were made for each direction by each observer. The closing of the left ear considerably lessens the accuracy even on the right side.[1]

The tests were then continued on the left side and the first direction tested was 45°lb. The surprising result was that the observers were wrong in almost every answer. They felt sure that they clearly noticed a difference between the directions and yet their answers were consistently reversed in nearly every case. It was thought that this might be due to some peculiar reflection from the walls of the room. The experiments were, consequently, repeated out of doors on four observers, giving the following results:

TABLE X.

	0°f	45°rf	90°r	45°rb	0°b	45°lb	90°l	45°lf
F. O. S.	4.8	14.3	13.6	8.6	4.8	14.3	20.0	21.8
H. S. B.	3.4	29.0	14.3	12.0	2.9		20.0	7.5
D. S.	4.8	19.0	4.1	3.4	7.3		16.0	8.0
G. P. K.	3.4	8.0	8.0	10.2	4.0		23.3	5.8
Averages	4.1	17.6	10.8	8.5	4.8	(14.3)	19.8	9.0

Twenty-five determinations were obtained from each observer for each direction. The results are very similar to the indoor tests. Three places are left blank under 45°lb because there the same characteristic reversals occurred that were noticed in the indoor tests. None of the observers except D. S. knew anything of this observation before the experiments. Evidently it was not due to reflection of the sound from the walls.

The explanation undoubtedly is that under normal conditions when both ears are in use a sound in front of the standard, 45°lb, seems stronger than one back of it. But when the left ear is thrown out of activity the sound that then seems stronger is the one back of 45°lb, but according to the habitual method it would be located in front of the standard, and the outcome is the reversing of the actual positions of the stimuli because of the reversal of the data of localization. Similar reversals might

[1] Cf. Figure 3 in Part I.

be expected to occur in the left front quadrant but for unknown reasons they did not occur.

One observer did not reverse his answers but made the same observation as the others. Instead of placing the stronger sound in front of the standard he placed it back, evidently for the reason that he was conscious of the fact that the left ear was closed and consequently he thought the stronger sound must be the one nearest to the right ear. The introspection of another observer on the same point corroborates this and clears up the matter of reversals. He remarked: "The stronger sounds I call forward and those weaker back. [This of course reversed the actual positions.] That is the way they seemed when I had my attention on the left ear, but if I placed my attention on the right ear, that is, if I made myself conscious of the fact that I was using only the right ear, I would tend accordingly to correct myself and call the weaker ones forward and the stronger ones back." The other observers had no suspicion of reversals and assumed that most of their answers were correct.

In the next group of tests the aim was to determine the accuracy of localizing the twenty-four standard directions in the horizontal plane through the aural axis according to the method described in Series III. Table XI contains the results.

Table XI.

Monaural.

Number correct on the right side: 11 out of 44, or 25%	63.6%
Number on right side misplaced 15°: 17, or 38.8%	
Number of confusions on right side: 4	
Number correct on left side: 3 out of 44, or 6.8%	22.8%
Number on left side misplaced 15°: 7, or 15.9%	
Number of confusions on left side: 12	

Binaural.

Number correct on both sides: 32 out of 88, or 36.4%	81.2%
Number on both sides misplaced 15°: 43, or 44.8%	
Number of confusions on both sides: 10	

The distribution of cases was about equal for the two sides.

These tests were made on four observers, each one going through the series of twenty-four standards, once monaurally

and once binaurally in order to compare the two. In the monaural tests the left side has a much smaller percentage of correct or approximately correct localizations than the right side—22.7% against 63.6%. It has also more confusions—12 against 4. In the binaural tests 81.2% are correct or approximately correct as compared with 63.6% on the right side in the monaural tests, showing that the exclusion of the left ear by artificial means affects the right side to a material extent.

One Ear Partly Defective.

The same two sets of tests were made upon an observer whose left ear had been defective for several years. Table XII gives the results of the discrimination for directions, and Table XIII for the localization of the twenty-four directions.

TABLE XII.

	0°f	45°rf	90°r	45°rb	0°b	45°lb	90°l	45°lf
Out of doors	4.0	14.5	19.0	29.0	5.8	25.3	17.0	12.0
In doors	7.3		14.5		8.0			

TABLE XIII.

In doors. Right side.

Number correct: 1 out of 11
Number misplaced 15°: 4
Number of confusions: 1

Left side.

Number correct: 1 out of 11
Number misplaced 15°: 2
Number of confusions: 1

Out of doors. Right side.

Number correct: 2 out of 11
Number misplaced 15°: 1
Number of confusions: 5

Left side.

Number correct: 0
Number misplaced 15°: 4
Number of confusions: 1

The localization is poorer than for the artificially monaural observers, particularly in the results of Table XIII. But there is practically no difference between the two sides, which can be accounted for by two reasons. First, the actual difference in acuity between the two ears was not as great as the observer thought. The threshold as found by the audiometer was 22.6

for the right ear and 37.9 for the left ear. Second, the observer had been defective for a number of years and had accustomed himself to the difference, whereas in the case of the artificially monaural observers the change was sudden.

Completely Monaural Observers.

The two observers, O and B, employed in these experiments were completely deaf in the left ear. Most of the data which are presented were obtained from O, a university student who has been deaf in his left ear since infancy but whose right ear has normal acuity, 16.7 as measured by the audiometer.

B is a middle aged physician who has been deaf in his left ear for thirty-two years. The deafness was consequent upon cerebro-spinal meningitis. Careful diagnosis by an otologist

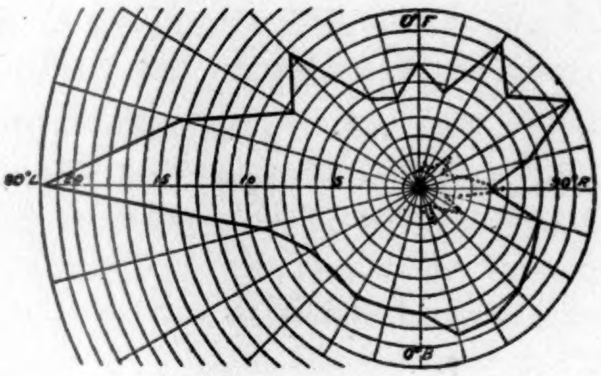


FIG. 9

showed that there is a lesion in the left auditory nerve. Otherwise the mechanism of the ear seems to be in perfect condition. The data obtained from this observer are in Table XIV.

Discrimination for directions—The first series of tests had in view the measurement of the discriminative ability between directions. The same method that has been followed in all the discrimination tests was followed here. The measurements were made out of doors.[1] The curve in Figure 9 contains the results obtained from O. It is based upon 1200 judgments, fifty for each of the twenty-four directions in the horizontal plane through the aural axis.

[1] The apparatus was a simplified form of the sound perimenter, consisting of only one arm and a scale. This was more convenient for outdoor experiments than the perimeter.

The main facts embodied in the curve are: (a) The most accurate region is at 90°r, just opposite the intact ear. A change in direction of four degrees can be detected there. (b) The most inaccurate region is at 90°l, opposite the deaf ear. A change of twenty-two degrees is necessary there to be noticeable. (c) In all the other directions the accuracy of localization is practically the same.[1]

This curve may be regarded as the typical monaural curve, comparable with Figure 3 in Part I as the typical binaural curve, which is represented by the dotted line in this figure.

There are, however, several interesting contrasts between monaural and binaural localization. In binaural localization 0°f and 0°b are the most accurate regions, and 90°r (or 90°l) is the poorest. In monaural localization, on the other hand, 90°r (or 90°l, in persons deaf in the right ear) is the most accurate region, while 0°f and 0°b are very much poorer. The accuracy at 90°r is about the same for a monaural as for a binaural person. This corroborates the statement[2] that in the vicinity of the aural axis the perception of direction depends almost entirely on one ear even in a binaural person.

While the monaural curve reveals some very interesting features and presents a remarkably symmetrical appearance, there is really nothing surprising about it. Its form might almost have been predicted on the basis of the facts known about binaural localization. We should expect the keenness of localization in the region near the intact ear to be the same in monaural as in binaural localization, because there the coöperation of the two ears is at a minimum. We should expect this to be the most accurate region because it is nearest to the active ear. Localization on the side of the deaf ear must necessarily be crude and unreliable because the sounds do not reach the intact ear directly. We should also expect the monaural localization to be much inferior to binaural localization in front and in the back, be-

[1] A few skirmishing tests on B revealed the same facts. His localization at 90°r was quite accurate, fully as accurate as in a normal individual. A change of five degrees could be detected. On the left side localization was quite poor.

2 See Part I, p. 24.

cause the great accuracy in those directions in binaural localization is due to the maximum coöperation of the ears.

What are the data upon which monaural perception of direction depends? Both observers were careful in their introspective analysis and agreed closely in their statements. According to observer O, from whom most of the introspective accounts were obtained, the perception of direction depended upon two factors, viz: intensity and his 'sense of direction.' When he was asked why he located a sound in a particular place he would generally attribute it to greater or less intensity than the sound had when it came from another direction. If intensity did not seem to be the means of recognizing the direction, which was seldom, he would reply that he did not know just why he located the sound at that place. And if he was further urged to analyze the basis of his judgment, he would simply reply that his 'sense of direction' told him that it was there. The term 'sense of direction' is undoubtedly an abbreviated expression for a number of unconscious elements, perhaps mostly qualitative characteristics, which were so interwoven in the perceptive process through habit and experience that the observer was unable to disentangle them.

The intensity factors were by far the most important, in the estimation of the observer, but probably not as important as he considered them to be. It is, however, true that in his localizations he seemed to depend very consistently upon the variations in intensity.

He had a sort of system of relative intensities which may be described as follows. There were two poles, one of maximum and the other of minimum intensity, located on opposite sides of the head. The pole of minimum intensity, at which the sound seemed weakest, was at 90°l, opposite the deaf ear; and the pole at which the sound seemed strongest was apparently not at 90°r but just a little in front of it, at 75°rf. All other directions in the circumference between these two poles were distributed according to a regular scale of diminishing intensities from right to left. The observer's judgments seemed to depend almost wholly upon these intensity differences. In determining the position of a sound with reference to the stand-

ard he would locate it on the right if it seemed stronger than the standard, and on the left if it seemed weaker. In all the standard directions from 75°lf to 60°rf in the anterior quadrants, and from 75°lb to 90°r in the posterior quadrants, the sounds on the right of the standard seemed stronger than those on the left of the standard. At 90°l where a sound in front of the standard would have the same intensity as a sound an equal distance back of it, the discrimination between the directions was very crude. Both the one in front of the standard and the one back of it seemed stronger, and consequently it was almost impossible to distinguish between the two. At the pole of maximum intensity on the right side the same conditions would obtain, but it seemed that the qualitative data were more conspicuous in the perception of direction.

In a normal binaural person the monaural process is narrowly limited to the immediate vicinity of the aural axis. The monaural and binaural curves show equal accuracy only at 90°r. At 75°rf and 75°rb monaural localization is poorer.

The results of Bloch,[1] who has made similar experiments on the discriminative ability in the horizontal plane through the aural axis, are different. The only respect in which the curve in Figure 9 agrees with Bloch's curve is in the greater accuracy on the side of the active ear. Otherwise his curve is rather irregular.

The difference between the two curves is probably due to two reasons. In the first place, Bloch's observer was not strictly monaural. Monaural conditions were produced by closing the left ear. This does not insure absolutely monaural localization, and the sudden change by bandaging one ear does not give sufficient time to the individual to adjust himself to the new situation. The results in Table XI obtained under artificial monaural conditions, are quite different from the results of Figure 9. The observer upon whom the experiments here reported were made, had been deaf in the left ear since infancy. Secondly, Bloch's curve is based on only one third as many measurements as the curve in Figure 9.

[1] Bloch, "Das Binaurale Hören," 35.

Localizing the twenty-four directions—The twenty-four standard directions were given in chance order and the observer located the sounds. Table XIV contains the results.

TABLE XIV.

Positions of sound.	Judgments O		B	
0°f	15°lf	15°lf	30°rb	45°lb
15°rf	30°rf	30°rf	15°rf	15°rf
30°rf	15°rf	15°lf	60°rf	60°rf
45°rf	30°rb	30°rf	45°rf	75°rb
60°rf	45°rb	30°rf	45°rf	60°rb
75°rf	30°lf	45°rf	45°rf	60°rf
90°r	75°rb	90°r	90°r	45°rf
75°rb	75°rb	60°rf	60°rb	0°b
60°rb	45°rf	45°lf	90°r	90°r
45°rb	30°rb	30°rb	45°rf	60°rb
30°rb	45°lf	60°lf	90°r	30°rb
15°rb	30°rb	75°rb	45°rb	75°rb
0°b	15°rb	15°rb	30°lb	45°rb
15°lb	0°b	45°lf	0°b	45°lb
30°lb	45°lb	30°rb	0°b	0°b
45°lb	45°lb	15°lb	45°lf	30°lb
60°lb	30°lb	30°lb	45°lb	60°lf
75°lb	45°lb	30°lb	90°l	45°lb
90°l	60°lb	0°b	45°lb	45°lf
75°lf	75°lf	90°l	90°l	90°l
60°lf	60°lf	45°lb	45°lb	90°l
45°lf	90°l	45°lb	60°lf	60°lb
30°lf	60°lb	45°lb	45°lb	45°lb
15°lf	15°rb	75°lb	30°lf	45°lb

Total correct on right side: 7 or 15.9%
Misplaced 15° on right side: 12 or 27.3%
Confusions on right side: 6

Total correct on left side: 3 or 6.8%
Misplaced 15° on left side: 11 or 25.0%
Confusions on left side: 9

Total correct: 10 or 10.4%
Total misplaced 15°: 27 or 28.1%

The contrasts between the right and the left side, and between monaural and binaural localization, are clearly brought out.

The right side has more correct localizations (15.9%) than the left side (6.8%). The number misplaced 15° is nearly the same on both sides. The left side has more confusions, 9, than the right side, 6, implying that the localization is less reliable on the left side.

Comparing these figures with the same tests on normal binaural observers (upper part of Table VI), we notice that binaural localization is considerably better. In the latter 93.7% are correct or misplaced 15° as compared with 38.5% in the former. Another significant contrast is the fact that monaural localization has numerous confusions of the right with the left hemisphere. For example, 75°rf was placed at 30°lf. O made seven such confusions in 48 trials, while such confusions scarcely ever occur in a normal person. Only two such cases occurred in 864 trials, Table VI.

The monaural observers were also much more reluctant in giving their judgments. Many trials had to be repeated several times, which was seldom necessary with the binaural observers.

The data of localization seemed to be the same as in the foregoing experiments, both observers agreeing that intensity was the most potent factor. When a sound seemed weak it was placed somewhere on the left side, and when it seemed strong and clear it was placed on the right side. The observers depended largely upon the relative intensities of the successive stimuli. On the left side the sounds seemed fainter and farther away, while on the right side they seemed stronger and clearer. O frequently said, "This sound is stronger than the one before, it must be on the right side," or "This sound is fainter, it must be on the left side." B stated when the sound was given at 60°lb, "This is a good deal farther away than the preceding sound" (which had been at 45°rf). He located it at 45°lb.

Elimination of intensity—Since intensity seemed to play such a large rôle in the perception of direction, a special set of experiments was planned to determine how important a factor it was. An attempt was made to eliminate the characteristic intensity differences. The telephone stimulus of the sound perimeter was carried through the audiometer by means of

which the intensity could be accurately controlled. By a few preliminary trials a stimulus of intensity 35 (audiometer scale) at 90°r was judged to be equal to intensity 38 at 90°l. On this basis the stimuli from the directions between these extremes were graded, and the intensities were distributed accordingly among the directions as follows:

From 45°rf to 45°rb intensity 35 was used.
From 30°rf to 0°f and from 30°rb to 0°b intensity 36 was used.
From 15°lf to 45°lf and from 15°lb to 45°lb intensity 37 was used.
From 60°lf to 60°lb intensity 38 was used.

The observer was told that the strength of the sound would be different in different directions. Each of the twenty-four directions was given twice in chance order, yielding the following results. Of the forty-eight trials only three were correct, and eleven misplaced 15°, that is 29.2% were correct, or approximately correct, while in the same tests with uniform intensity, Table XIV, O had 41.6% correct or approximately correct.

Then the "discrimination" tests were repeated on Observer O at 45°rf, 45°lf, 45°lb and 45°rb. In each of these directions 100 trials were made, 50 in which the intensity (35) was unchanged, and 50 in which three intensities (33, 35 and 37) were used in chance succession. For example, the intensity at the standard direction might be 35 and the intensity at the side might be either 33, or 35, or 37.

Table XV.

45°rf	45°lf
Difference 10°	Difference 10°
Intensity uniform	Intensity uniform
74% correct	86% correct
Intensity varied	Intensity varied
64% correct	78% correct
45°lb	**45°rb**
Difference 15°	Difference 10°
Intensity uniform	Intensity uniform
84% correct	78% correct
Intensity varied	Intensity varied
58% correct	76% correct

These measurements clearly demonstrate that intensity is a very important factor in localization. When the intensity was varied so that the observer could not rely upon a certain intensity to indicate a certain direction, the localization became considerably inferior—on the average 12%. But intensit probably did not play as important a part as the observers thought. Otherwise, the localization, in which the different intensities were used, should have been even more inferior. The fact that the observer was able to localize with some certainty the sounds of varying intensities, indicates that qualitative factors played a considerable part.

GENERAL CONCLUSIONS

The results and conclusions may be stated under three heads, (a) a summary of the specific facts demonstrated by the experiments of Part II, (b) a summary of the factors which enter into the localization of sound, and (c) the bearing of these results upon the traditional theories of localization.

Summary of the Results in Part II.

1. The threshold of hearing is considerably lower, that is, sensitivity is keener, at the side than either in front or in the back.

2. There are two types of observers: Those whose threshold is lower in the front than in the back, and those whose threshold is lower in the back than in front.

3. The ratio of sensitivity of the side to front (or back) is the same irrespective of the absolute threshold. This ratio is approximately 3:4.

4. The fact that the threshold is lower at the side than in front or in the back means that a given sound will seem nearer and more intense at the side than in front or in the back because it is relatively so much higher above the threshold. In other words, the nearer a sound is to the aural axis the stronger and clearer it seems, and this apparent change of intensity with change in direction is a potent factor in the localization processes.

5. Pitch discrimination is decidedly poorer at the side than in front or in the back.

6. Discrimination for intensities of a sound is about uniform for all directions.

7. The richer and more complex a sound the more accurately it can be localized.

8. A pure tone, so far as it has been approximated, can be localized, although with much less accuracy than a complex tone.

9. Angular differences between directions are overestimated in the frontal region, and underestimated in the lateral region.

10. Monaural localization is considerably inferior to binaural localization. The most accurate region is opposite the intact ear, and the most inaccurate region is opposite the deaf ear. The nearer a sound is to the active ear the stronger and clearer it seems.

Summary of the Factors in Localization.

The experimental evidences gained in this entire investigation warrant, I believe, a division of all the perceptual data involved in the localization of sound into two classes, namely, intensity and quality.

I. INTENSITY.

The intensity factors are again of two kinds:

1. The Binaural Ratio of Intensities.

The localization of sound depends to a marked extent upon the relation between the intensities with which a sound strikes the two ears. (See Part I, p. 15.) Some of the experimental evidences for this are:

a. The accuracy of localization is greatest where slight changes in this ratio are most readily perceived, that is, in front and in the back.

b. Localization is poorest where changes in the ratio are not so easily perceived, that is, on the sides, in the region of the aural axis.

c. The presence of confusion points or directions in symmetrical positions for which the ratio is the same.

d. The difficulty of median plane localization, which is really a particular case of confusion points. The ratio is the same for all directions in the median plane.

e. The inferiority of monaural localization in which the ratio is entirely absent.

2. *The Monaural Ratio of Intensity.*

There are systematic differences in the intensity of a sound when it comes from different directions. Evidences for this are:

a. The threshold measurements (Part II, Series I), showing that the threshold is lowest in the region of the aural axis and highest in front and in the back, consequently a sound at the side seems stronger than in front or in the back.

b. The "distance" tests (Part I, p. 21), showing that a sound is estimated to be nearest in the region of the aural axis.

c. The introspections of the observers in all the localization experiments, and especially in Tables I to III and V to VII in Part I.

d. Monaural localization depends to quite an extent on the relation of the intensities for different directions, which is brought out distinctly by the introspections of the observers, and by the tests in which stimuli of different intensities were used, resulting in poorer localization.

e. In the artificially monaural tests the uniform reversing of the responses at 45°lb indicates the force of the variation of intensity with direction in the perceptive process.

II. *QUALITY.*

The localization of sound depends to a considerable extent upon the quality and complexity of the sound. The evidences for this are:

a. Complex sounds, such as the human voice, noises and the telephone sounds, can be localized much more accurately than

(relatively) pure tones, such as the singing flame and resonator tones. (Part II, Series III).

b. The introspective accounts of the observers testify to the importance of qualitative changes and signs for different directions. (See Tables I to III and V to VII in Part I.)

c. The fact that median plane localization is possible to some extent.

d. In monaural localization the elimination of the intensity factor still left a considerable accuracy in localization, which must have been due to the qualitative data.

The Bearing of the Results on the Theories of Localization.

Of the four or five theories[1] which have been advanced at various times, the intensity theory has no doubt received the most support. It attempts to explain the localization of sound essentially by the binaural ratio of intensities. In the light of present results it is evident that this ratio can not alone account for the perception of direction. In fact it plays a relatively small, though significant, part in the complete process.

The quality and complexity of sound are real, potent factors in the localization process.

Intensity itself has been demonstrated to be effective, not merely in the binaural ratio, but in the characteristic changes and differences in different directions.

If localization depended entirely upon the binaural ratio, monaural localization would be an impossibility; and binaural localization itself would be considerably inferior. We would be surprisingly deceived by the systems of confusion points. Theoretically there are numerous planes parallel to the median plane in which the ratio is the same for all points in the same circumference in a given plane. But the other intensity characteristics and the qualitative factors come in to render accurate localization possible.

The traditional intensity theory is in the main correct, but it is quite inadequate. We must add to it the qualitative ele-

[1] Pierce, "Studies in Space Perception." p. 52.

ments and the monaural quantitative elements. These two have coördinate value with the binaural ratio, in the auditory perception of direction. With these additions, the theory would be more appropriately called the "intensity-quality theory."

ON THE TRANSFERENCE OF TRAINING IN MEMORY.

GEORGE CUTLER FRACKER, A.M., PH.D.

CONTENTS.

Researches in the transference of training, or "spread of ability," seek to determine the influence of practice in one activity upon our abilities in other activities. There are, in general, two methods by which this problem has been attacked—the method of correlation, and the method of direct experiment. The method of experiment was chosen for this study. The usual means of conducting research by the experimental method in the transference of training is to employ two sets or series of experiments; one set to be given before and after training; the other a training set, in which one may be practiced for some time. The experiments given before and after training may be called the test series, and the other set, the training series. The test series is made up of several experiments, some of which are like the training series, while others differ.

The object of these two sets of experiments is to discover what effect practice in the training set has upon the test set, in

which the observer is not trained. This effect is measured by the difference in the results between the test series given before the training, and the test series given after the training. In order to measure the amount of training in the test series itself, two sets of observers are used: one set who take both the test and training experiments and another set who take the test experiments only. The difference in the gain between those trained and those untrained indicates the influence of the training experiments. The observers composing these two sets are selected on the basis of similarity in age and ability.

The present research is devoted to a study of certain aspects of memory. The training series consisted in practice in memory for the order of four tones. The experiments of the training series and the manner of conducting them will be explained under "Training Series." The test series consisted of eight experiments, as follows:

1. Memory for poetry.
2. Memory for the order of four shades of gray.
3. Memory for the order of nine tones.
4. Memory for the order of nine shades of gray.
5. Memory for the order of four tones.
6. Memory for the order of nine geometrical figures.
7. Memory for the order of nine numbers.
8. Memory for the extent of arm movement.

The relation of the respective test series to the training series, in the above experiments, was as follows:

The four grays; different in content, same in method.

The nine tones: same content, different in method.

All the other tests: different in content and method.

This relation between the training and the test experiments was planned in order that the elements concerned in transference might be determined by analysis of the final results. In order to aid still further in this analysis, each observer was asked to write a careful introspection at the close of each day's training, after each experiment of the test series, and a general introspection at the close of the experiments giving his observa-

tions and conclusions concerning the essential elements in improvement and transference. The object of the introspection was to discover the method or lack of method of each observer. Every effort was made to guard against giving the observer any hints as to how to perform the memory part of the experiments. At the beginning of each test, observers were given written instructions describing just what they were expected to do in response to the stimuli, but conveying no information as to how to do it.

The test experiments present the following important advantages: (1) Each is a task sufficiently difficult to demand intense application while it lasts. (2) Each test is so brief that it affords a minimum amount of training within itself. (3) The time between the first and second tests is fairly long. (4) The tests similar to the training series were taken in double fatigue order. (This order is explained under "General Experimental Conditions.") (5) The test material is of such nature that the second test could be made exactly equivalent to the first test without being a repetition of it.

DESCRIPTION OF THE TEST SERIES OF EXPERIMENTS

Experiment I. Memory for Poetry. Two stanzas of "Eve of St Agnes." The observer memorized two stanzas of this poem so that he could repeat each aloud to the experimenter without error. A record was kept of the time taken for each stanza, and of the errors made.

Experiment II. Memory for the order of four shades of gray. This experiment consisted in exposing before the observer, numbers two, seven, thirty and forty-five of the Hering Grays, by means of the psychergograph. The psychergograph consists of two main parts, the stimulator and the recorder. As the recorder was not used in these experiments, the description of it is omitted here. The following partial description of the stimulator is quoted from the original description by Seashore, Univ. of Iowa, Studies in Psychology, III, p. 5.

"The stimulator is a plain case, 40 cm. square, with a slanting cover. Near the front edge of the cover is a signal window, 8 millimeters wide by 20 milli-

meters long, through which the signals are seen. One hundred signals are pasted or printed on a paper disk, 38 cm. in diameter, so that when the disk revolves they are seen singly in succession right back of the signal window. The paper disk is clamped on a metal wheel which has fifty teeth on its edge. This wheel is energized by a strong clock spring which revolves it and the disk one one-hundredth of a revolution, thus exposing a new signal every time the detent which holds it is released. This detent is in the form of a lever escapement and is operated by electro-magnets." * * *

"A circle of the revolving disk is seen through the cover. On this there is a cross line which passes before the circular scale of a hundred units and indicates to the experimenter which signal is in view. This indicator serves at once as a counter of the number of acts performed and as a guide for the beginning and ending of the series. The order of the signals is determined in the making of the series, either by chance or by some suitable system. The experimenter, therefore, knows the actual sequence of the signals in every series, but the observer has no means of knowing at any time what signal shall appear."

The stimulator was arranged to expose each of the four gray disks for one-half second, an interval of one-half second being allowed between each exposure. Each disk was seven mm. in diameter. After the four grays were exposed, a blank remained before the observer for four seconds, then another arrangement of the four grays was given and another blank exposed. When the second blank appeared, the observer responded, giving aloud the order of the first group of four grays. After the third group had been exposed, he responded to the order of the second group, and so on through the series. The series consisted of forty groups taken in double fatigue order. In responding to the order of the four grays, the observer called the darkest gray, 4; the next lighter, 3; the next lighter, 2; and the lightest, 1. The grays were given in twenty-two different mutations, but the orders 1, 2, 3, 4, and 4, 3, 2, 1 were not used.

Experiment III. Memory for the order of nine tones. The third set of stimuli used consisted of four tones, varying in intensity, and delivered to the observer through a telephone at the rate of one tone per second, each stimulus sounding one-half second. The four sounds were produced by placing a telephone receiver in circuit with a 100 v.d. electro-magnetic fork and branching the circuit through four lines of resistance so adjusted as to produce four readily distinguishable tones when

the four keys of these branches were closed in turn. The e. m. f. was kept constant. These four tones were arranged so as to form a group of nine tones. After the delivery of the stimulus group, an interval of nine seconds was interposed, during which the observer responded aloud to the order in which the nine tones had been given. The tones were called 1, 2, 3, 4, in the order of loudness, 4 being the loudest. The 1, 2, 3, 4 and the 4, 3, 2, 1 orders were avoided. Twenty groups of nine tones each were given in double fatigue order.

Experiment IV. Memory for the order of nine grays. In this experiment, the four grays of Experiment II, were so arranged as to form a group of nine grays. These were exposed on the stimulator at the rate of one per second; exposure, one-half second. After each group of nine grays had been exposed, there followed an interval of nine seconds, during which the observer gave aloud the order of the grays in the group just given. Twenty groups of the nine grays were given in double fatigue order.

Experiment V. Memory for the order of four tones. The four tones were those composing the major chord on the piano. Instead of responding by number, the observer responded by the names Do, Me, Sol, Do–2. The tones were produced in the same order, with the same duration, at the same rate and with the same response intervals as the grays in Experiment II.

Experiment VI. Memory for the order of nine geometrical figures. The nine figures drawn upon a card are described as follows:

> "Each figure is composed of three lines; the lines are all straight; two lines are equally long, and the third is half as long as these; the two long lines always adjoin each other; the lines join either at the end or in the middle; no line is crossed; no two figures are alike; the angles are right angles." [1]

The stimulus card was exposed ten seconds, and the observer was given one minute in which to reproduce all the figures he could remember, drawing them in the same relative positions and proportions as on the card. Five records were taken.

Experiment VII. Memory for the order of nine numbers.

[1] Seashore, "Elementary Experiments in Psychology."

The stimulus for this experiment was nine numbers of two figures each, read aloud at the rate of one pair each second and a half. After nine pairs had been read, the observer was given fifteen seconds in which to write as many of the pairs as he could remember. Ten sets of nine pairs made up the test.

Experiment VIII. Memory for the extent of arm movement. The apparatus for this experiment consisted of a glass rod mounted one-half inch above a metric scale. A hard rubber cylinder, about one inch in diameter, was fastened firmly at one end of the glass rod so that its edge tallied with the zero point of the metric scale. Upon this glass rod was a second hard rubber cylinder freely adjustable. The observer, with eyes closed, moved his finger with a free arm movement along the glass rod, from the fastened cylinder to the adjustable cylinder, held at a standard point by the experimenter. The observer was allowed to move the finger out and back twice. The experimenter then moved the adjustable cylinder away from the standard position, and the observer moved his finger along the rod until he thought he was reproducing the standard distance. Three standards were used; viz: fifteen, twenty, and twenty-five cm., and ten trials were taken in varied order for each standard.

DESCRIPTION OF THE TRAINING SERIES OF EXPERIMENTS

The apparatus for the training series consisted of that described in test Experiment III, the nine tones. The four intensities of tone of the fork were arranged in all possible combinations except the 1, 2, 3, 4, and the 4, 3, 2, 1, order. Enough of these combinations were used and repeated to make a series of seventy-five groups. The observer, seated comfortably at a table with a telephone receiver carefully adjusted to his ear, listened to the four intensities of sound. The experiment was carried on in exactly the same way as the four grays of the test series except that tones were used instead of grays and the number of groups extended to seventy-five, four sets of which made up a day's practice. It was possible to give seventy-five groups in about ten minutes. At the end of each ten minutes, a rest of one minute was taken.

For three observers, Tuesdays, Thursdays and Saturdays of each week were the practice days; for five, Tuesdays and Thursdays, or Wednesdays and Fridays, of each week.

METHOD OF RECORDING AND OF ESTIMATING RESULTS.

The observer's responses were kept by a recorder who used mimeographed sheets corresponding to the sheets used by the experimenters as a guide in giving the stimulus. These mimeographed sheets consisted of columns of numbers corresponding to the order of the numbers of the grays or tones used in the test and training series. The order in which the groupings were given was readily changed by beginning at different parts of the sheet. If the observer omitted to respond to any stimulus group, a line was drawn through that number upon the record sheet. If he responded incorrectly, his reply was written above the corresponding number on the record sheet.

Results in both the test and the training series are estimated on the basis of the per cent of correct responses. Training curves are plotted to show the per cent of correct responses in each of the four sets of seventy-five groups. Test results are shown by lines drawn across the charts.

GENERAL EXPERIMENTAL CONDITIONS.

Every observer was allowed a short preliminary practice at the beginning of each test or training period with the grays and the tones in order to secure adaptation. This seldom took more than a few trials.

No observer knew of the results of his records until after the experiments were entirely completed, with the single exception of G. C. F., during his second training series.

Every effort was made to preserve uniform conditions, especially for the two test series. A record was kept of the hour of the day when each experiment was taken; and the same day of the week, and the same hour of the day, were kept for each observer. Great care was used to keep the temperature, light, and sound conditions of the room as constant as possible. The

experiments were carried on in a room where the apparatus remained in the same position, and all the above elements could be easily controlled.

In order to determine the amount of improvement due to training, the observers were divided into two classes: (1) Those who took both the test and training experiments, and (2) those who took the test experiments only. Of the first class there were eight, and of the second class, four.

The following order was maintained in giving the experiments in the test series:

1. Poetry, two stanzas.
2. Four Grays, twenty groups.
3. Nine Tones, ten groups.
4. Nine Grays, ten groups.
5. Four Tones, twenty groups.
6. Geometrical Figures, five trials.
7. Nine Numbers, ten columns.
8. Arm Movement, ten trials for each of three standards, 15, 20, and 25 cm.
9. Four Tones, twenty groups.
10. Nine Grays, ten groups.
11. Nine Tones, ten groups.
12. Four Grays, twenty groups.

This arrangement gives a double fatigue order for the four experiments most closely resembling the training series.

The observers. G. C. F. is a teacher of psychology and has carried on experiments in this subject on two former occasions. With Professor Seashore, he devised the experiments of this series, set up the apparatus for the tests, worked out the scheme for the mimeographed record blanks, and served as experimenter and recorder for several of the observers. He is considerably older than the other observers. D. S. was also a trained observer and an instructor in psychology; thoroughly familiar with the material and method of this experiment. Nearly two years previously, he had been trained in experiments almost identical with this series[1] as regards stimuli and

apparatus, but the responses had been made by signals on four keys, instead of speaking. F. S. was a graduate student in psychology, trained in many forms of psychological experiment. E. M. C., H. C. E., A. R. F., M. M. M., and M. L., were college juniors with some experience in psychological observation. These observers took both the training and the test experiments.

J. W., M. C., M. D. F. and D. D. W. took only the test experiments; J. W. was a senior in college and somewhat older than the others. M. C., M. D. F., and D. D. W. were college juniors and all were familiar with psychological experiment and observation.

RESULTS OF THE TRAINING SERIES.

The practice curves and the curves of improvement in the test series are shown on Charts I to IX inclusive. All the observers except one were trained for four weeks, two or three days per week. G. C. F. was trained for eight weeks three times per week. Two charts, I and II, show the results for this observer.

Each point in the heavy curves represents the average for seventy-five groups of tones. Four such sets, of seventy-five each, represent a day's work. The curves are plotted on the basis of the per cent of correct responses. Each day's average is shown by a heavy horizontal line.

These curves show the progressive gain by practice in the training series and the relative ability in the end tests before and after the training.

G. C. F. thinks that the advantage he has had in his experience as experimenter is offset by the development of automatism which he was forced to break when he became observer. Before beginning the training, he had served as experimenter for three other observers, and had thus made about 10,000 reactions from the same stimuli but with attention upon the beat of the metronome, the delivery of the stimulus, etc., instead of upon the sounds of the stimulus. At the beginning of the training,

[1] SEASHORE AND KENT, "Periodicity and progressive change," Univ. of Iowa, Studies in Psychology, IV, p. 82 ff. (see practice curve opposite p. 85).

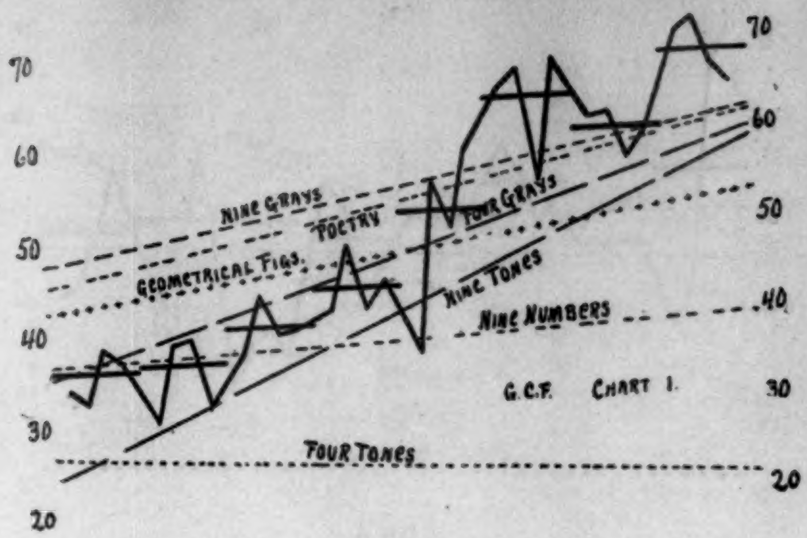


FIG. 1

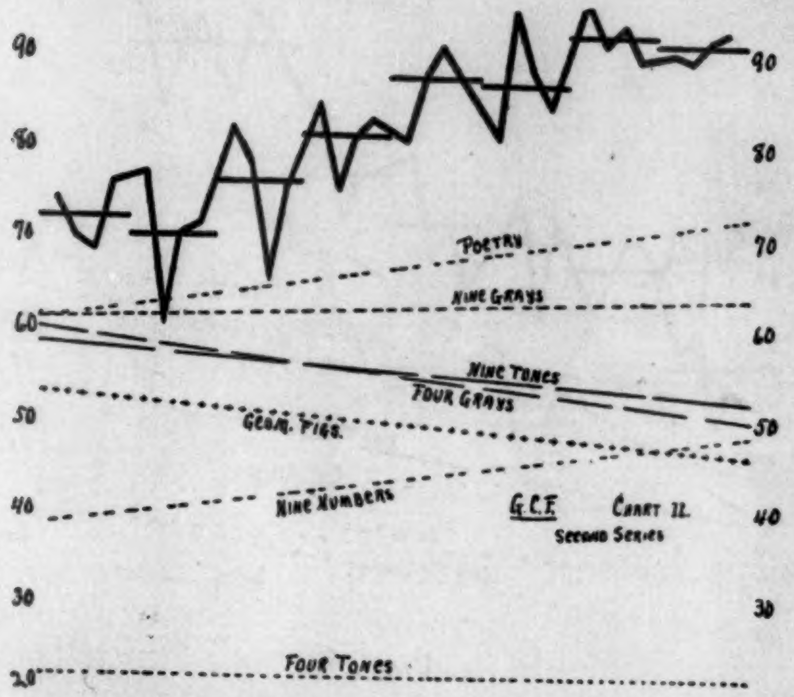


FIG. 2

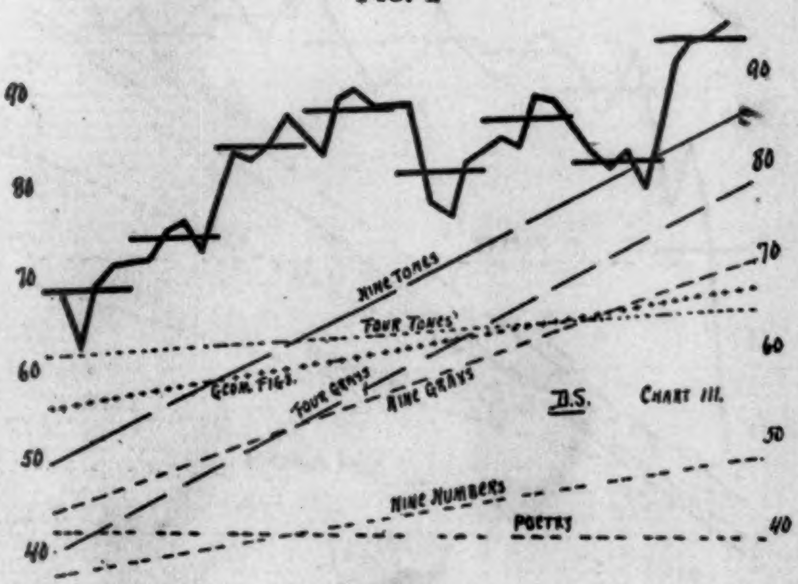


FIG. 3

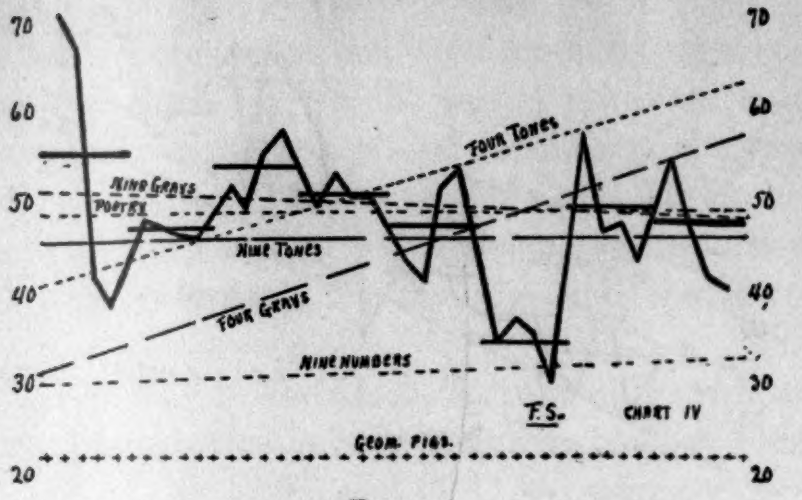


FIG. 4

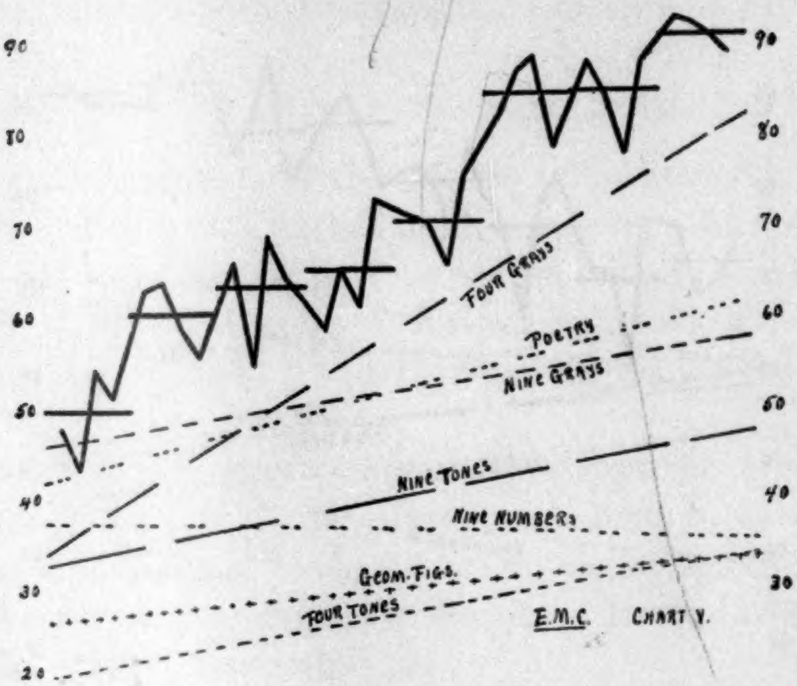


FIG. 5

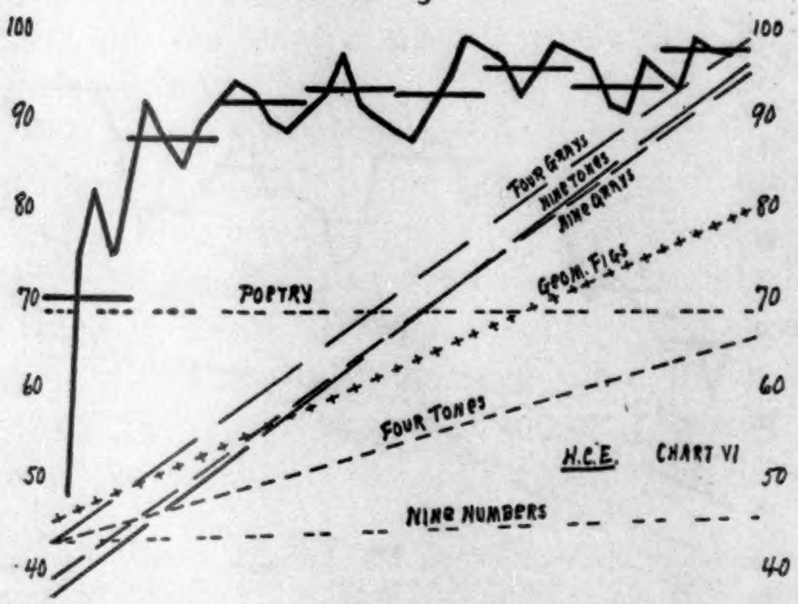


FIG. 6

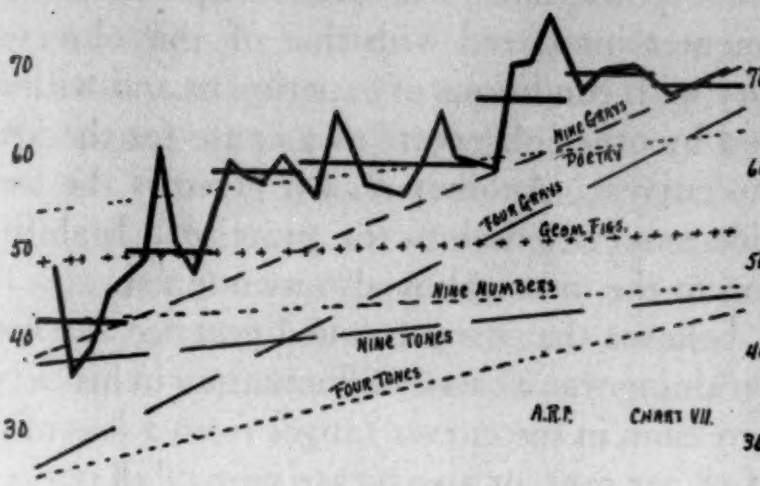


FIG. 7

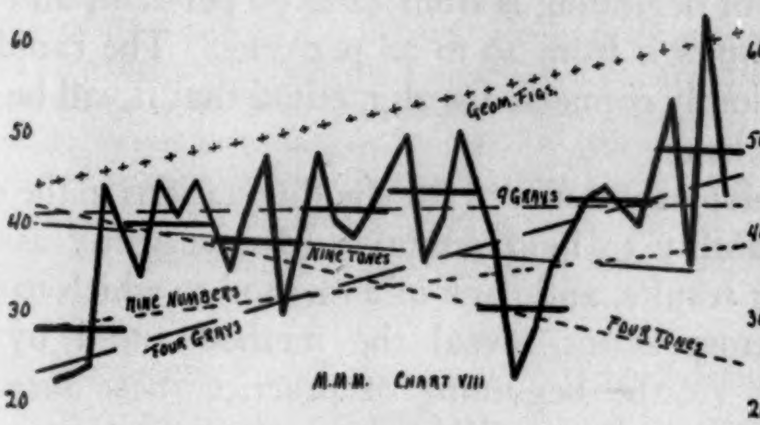


FIG. 8

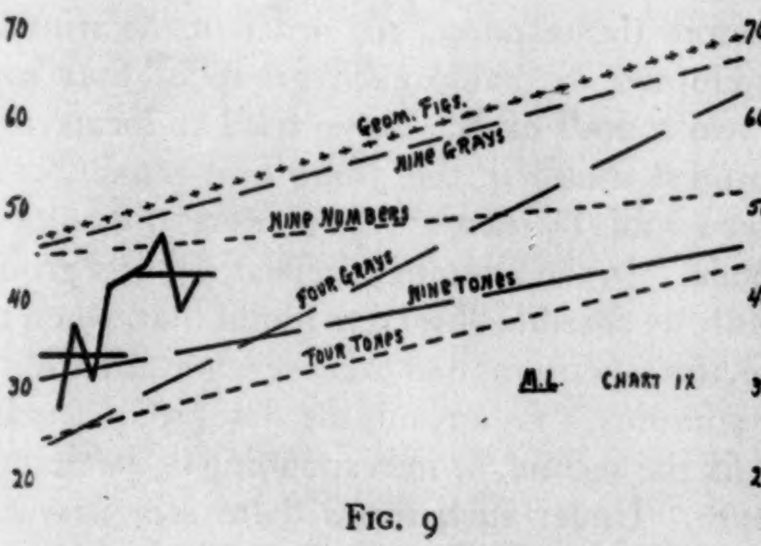


FIG. 9

this kind of response had to be broken up and the attitude of the experimenter interfered with that of the observer. Lack of familiarity with conditions of experiment and with apparatus is mentioned by other observers as a cause for the low starting point of the curves. Another reason given is the tendency to bring outside associations into the practice. Inability to hold the attention to the new task is also mentioned.

G. C. F. believes that his continued practice as experimenter during the training was a cause of fluctuation in his curve.

The progression in the curves ranges from a loss of 9 per cent to a gain of 48 per cent, or an average gain of 28 per cent. The average point at which the curves begin is at 50 per cent, and the average summit of the curves is 73 per cent. The range of the points of beginning is from 32 to 70 per cent, and the range of the summits is from 46 to 98 per cent. The rapidity of the rise is so closely connected with method that it will be discussed under that head.

The explanations offered for the fluctuations in the curves are mainly inability to hold attention, disconcerting associations, guessing at results, and lack of a method to which to adhere.

The introspections reveal the methods used by different observers. At the beginning of practice these introspections show that every observer did more or less guessing in responding. A number of devices were tried during the first days of training. All observers began by repeating as many times as possible, before the response, the order of the stimulus group. Several attempted to divide each group of four sounds into groups of two sounds each. Some tried to locate the loudest and the faintest sound in the group and remember the order of the other sounds by these. There were difficulties with all these methods. In the attempt to repeat the first group as often and as rapidly as possible observers found that, when the second group came, the repetitions had to cease while attention was given to the new stimulus. As a result the first group was lost in the attempt to fix the second, or in responding to the first the second was forgotten. Under such stress there is a strong tendency to guess, or to become confused. Both these things happened frequently. Several observers attempted to separate the stim-

ulus from the response. With G. C. F. this became a factor in his method. With F. S. there seemed to be a tendency to try new methods throughout the training. In guessing, some times the whole group is guessed at, other times one or two of the numbers of the group. All observers said that speaking in response disturbed the retention of the first group. After trying several schemes observers settled down to a definite way of retaining the groups and of making the responses. The individual methods adopted can be shown best by quoting from the introspections.

G. C. F: "The principal features of my method are: first, an imagery of position in space for the four tones. Number four is right at the ear, three is about four or six inches away from the ear, two is several feet away, and one is a faint sound a long distance off. The exact position of two and one is not clear, but the position of three and four is definite. Second, there is a separation in attention between the stimulus and the response, that is, attention is given to the first group, which is fixed by the imagery above described, and placed upon the tip of the tongue to be spoken immediately after the second group is heard. When the first group is thus delegated to the motor side, it is dismissed from active attention, which is then focused upon the second group. The second group is fixed almost while the first group is being automatically responded to."

With D. S. the method used goes back to his previous training. In the introspection following the first day's practice he said that many "names" used formerly came back during the hour and helped materially in the responses. The names referred to are the forms of imagery used by D. S. to identify different arrangements of the four sounds. On the second day of practice the system of "names came back completely," and he attributed practically all of his improvement to the recovery of his system. From thence he improved steadily.[1]

F. S. tried to remember the tones in groups of fours and to hold them by rapid repetition. After the second group was given he responded to the first group, and then immediately repeated the second group as often as possible before the next stimulus was given. Later he tried to remember the first two numbers of a group and guessed at the other two. When he missed a

[1] For full account of the method used by D. S. in his former training series see Seashore and Kent, op. cit. pp. 87–90.

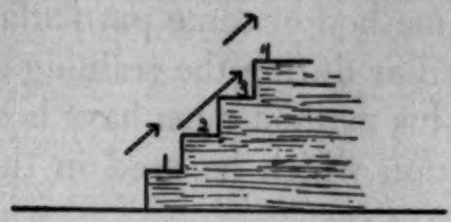
group altogether he guessed at it. Sometimes he grasped the first two numbers of a group with the last number and supplied the third. He found great difficulty in retaining one group while securing the next. Speaking in the response disturbed the retention of the second group. As practice proceeded he noticed no new method but seemed able to take things more quietly and to respond with less effort. He noticed especially a tendency to listen to the loud tone and to locate it in the group. This tendency results in a species of auditory imagery, but an imagery not definitely recognized. In the last introspections, the observer said that there was a tendency to locate the loudest and faintest tones in a group.

H. C. E: "In this training I used two methods of remembering. The first four numbers I repeated several times during the four seconds interval, and continued repeating them while the next four were being given; then I would give them aloud without giving any actual attention to them. While the second group was being given, I turned my active attention to it, first getting a visual image of their position and at the same time repeating the first four. In the four intervening seconds, I repeated aloud the first four members without any active attention to them, keeping my active attention on the visual image. Just as soon as the first four were repeated aloud and gotten out of my mind, I would immediately turn the visual image of the second group into a repeating image and wait for the next group of four. This same process was kept up throughout the experiment. The imagery is a vertical arrangement of the numbers of the four tones, in appearance somewhat like keys. The lowest number or key was 1, and the highest, 4. The keys were apparently numbered as though printed. Some groups, however, such as 4, 1, 2, 3, were divided into two, 4 being in a group by itself, and the 1, 2, 3 combined. The visual image is not an image of the numbers. They are arranged in vertical order and I pick them out vertically but do not see the numbers themselves. The one highest up or number four, is the largest, and number one is very small, with numbers three and two correspondingly between. As near as I can tell, they are round or disk-shaped and appear thus:

As practice continued the visual image became more familiar and found myself more and more able to remember the groups without giving much attention to them."

A. R. F.: "After repeating the numbers of the first group, I would form a sort of mental picture of how they would look on a scale, and this would help me in remembering them. When I got confused, I just stopped and did not

try to recall, but went on with the next group. The figure I find myself using after the third or fourth day of training is like the following:



There was a flight of four steps, each numbered in ascending order. This figure illustrates the group 2, 3, 1, 4; 2 and 3 being consecutive were represented by a long arrow and, because they came first, the arrow was close to the stairs. 1 came next and was a little farther away, and was a short arrow, being alone. 4, the last one, was still farther out, and was also a short arrow. I recalled the group by the position and direction of the arrows in the mental image. After listening to the second group, I go back and, from the picture, get the numbers in the order given. The mistakes I make are caused by taking too long to form the picture or to recall it."

M. M. M. developed a method of very vivid imagery rapidly. He says:

"In remembering these groups I thought of them as being in a position like four keys on a piano, such as C, D, E, F. I remembered them as 1, 2, 3, 4, and while one groupwas being given I tried to keep a picture of the order the keys took in the preceding group. It was much easier to remember the group when the numbers came together, such as 1, 3, 2, 4; in this case, I thought of 3, 2, as being between 1 and 4. I could see the keys just as if they were pressed down. In trying to remember a group, I sometimes hung to it too long and became confused in getting the next group. As practice continued this method became more reflex, and it was easier to remember the groups."

E. M. C. began the training by repeating to himself, about twice during the four seconds interval, the first group. Later the repeating became automatic, so also did the response; and he adds:

"I find that if I miss a group, that breaks into the rhythm and it is hard to get into the swing again. Sometimes I give a group and guess just to keep up the rhythm. During these tests I have been convinced that the subconscious does a great deal for us."

It seems that E. M. C. did not recognize a particular form of imagery as did the other observers. It is not clear from his introspections that he had an imagery form, nor is it clear that he did not have one. Throughout the introspections for the second test experiments, however, E. M. C. speaks of the aid

he received from the training series method in securing results in the test experiments. There seem some indications, therefore, that he had a method or some particular form with which he had become familiar during the training and which he used in the final test. This method must have been something more than that of repetition which he used in the first test experiments.

In the case of M. L. the training was carried on for two days only, and the introspections contain no definite statement concerning the specializing of method in the form of imagery. But under her introspection for the nine grays it is fully described. During the final test series she says that there was a decided help from the first test and the method of the training series. She says:

> "The loudest tone seemed to serve as a sort of station around which the others grouped themselves. This scheme helped very much in securing correct results. Sometimes the lowest tone served as a station. Such combinations as 3, 2, 1, 4, or 2, 3, 4, 1, were much easier, because of my way of remembering."

In the matter of recognition of a method, or what seems to be the same thing, the recognition of an individual imagery, the observers did not all recognize a particular method to such an extent as to describe it in introspections. From what has been quoted it is apparent that all but two, F. S. and E. M. C., recognize a peculiar imagery; and the evidence shows that they also used imagery but did not recognize it as such. The relation which the recognition of the method or imagery bears to improvement is significant. Without a conscious recognition of the imagery an observer may improve rapidly, or slowly, but a steady improvement seems to follow if an imagery exists, and is consistently used. E. M. C. seems to illustrate this. F. S. illustrates an observer who had imagery but who failed to use it consistently. With observers who recognized imagery the rate of the improvement seems to coincide with its recognition. This is reasonable for, as observers say, the recognition of a method gives one confidence in his ability to do the work. With G. C. F. imagery was recognized about the fourth or fifth day, and the rise in the curve is most rapid immediately after.

With D. S., imagery was recovered the first and second days and the rise in the curve is most rapid during the first, second and third days. With H. C. E., imagery was recognized the first day and the gain is very rapid the first two days. With A. R. F., imagery was described on the fifth and sixth days and the gain is greatest on the sixth day, although the gain is great on the second day also. In the case of M. M. M., whose curve shows only a slight gain during practice, yet who recognized imagery on the first day, the rise is great on the first day but is not great thereafter until the seventh and eighth days when he began to recover from an attack of the grip, from which he suffered severely on the sixth day.

With A. R. F., whose imagery seems somewhat intricate, the relation between complexity and rate of improvement is shown. This observer speaks of losing many groups because he had not time enough to adjust his imagery to the group.

In the case of untrained observers, a recognition of imagery is not alone sufficient to give this confidence of improvement; for many say that they are not sure that they can use the imagery in other tests. A certain familiarity with imagery would seem, therefore, essential. This familiarity the training series gives.

The fact then seems to be that steady improvement may take place because of the use of an imagery without a conscious recognition of its presence. An imagery may even be recognized without adding essentially to the speed of improvement, but a recognition of it adds confidence in one's ability and reliance upon one's method, which is pretty sure to result in rapid improvement.

The essentials of method in training as brought out by these experiments are; first, familiarity with conditions of training, such as the room, the light, heat, furniture, apparatus, the experimenters, the adjustment of the observer to the apparatus, and learning what is expected of the observer; second, the use of rapid and frequent repetition in order to retain; third, the sorting out of things essential to the performance of the act from those things that are non-essential;[1] fourth, the selection,

[1] See Coover and Angell, "General practice effect of special exercise," Am. Jour. Psychology, XVIII, pp. 328–341.

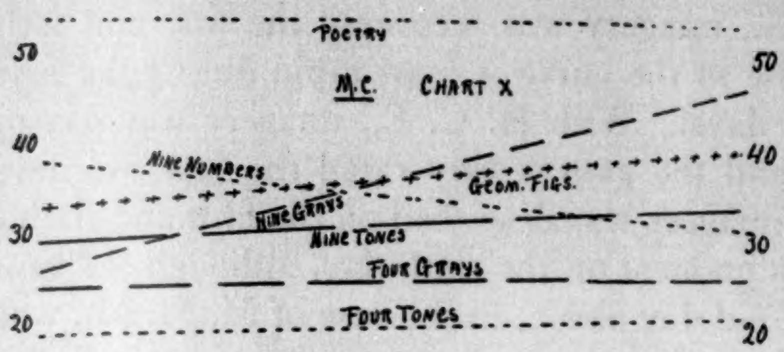


FIG. 10

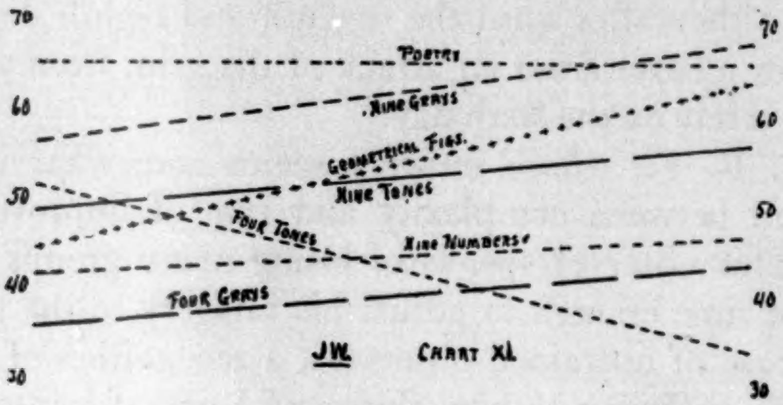


FIG. 11

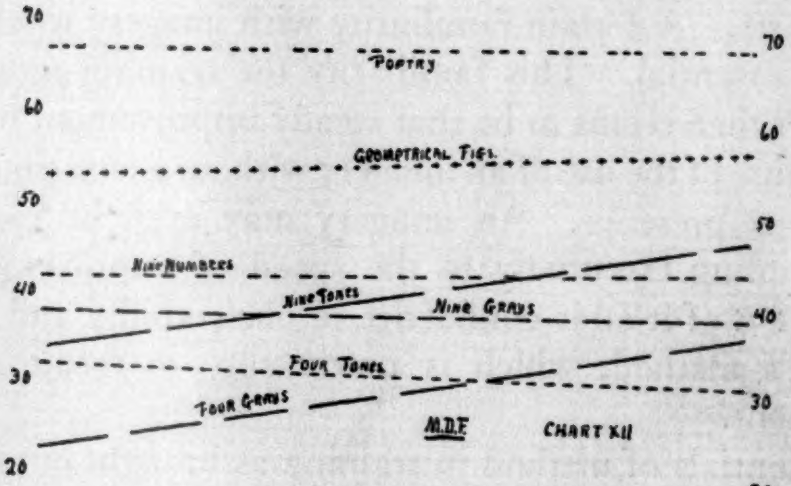


FIG. 12

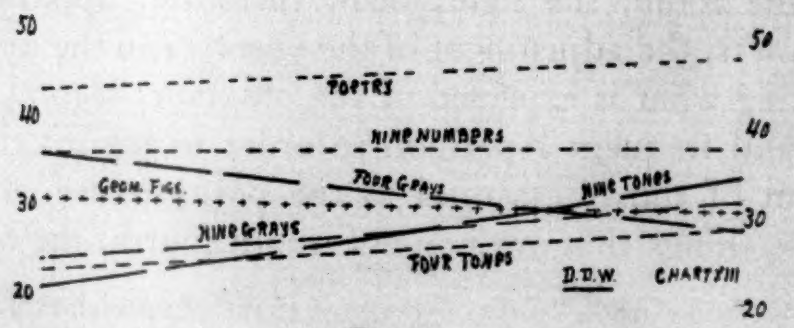


FIG. 13

TABLE I.

Results of Test Series for Trained Observers.

	G. C. F.						D. S.			F. S.		
	1″	2″	3″	2″—1″	3″—2″	3″—1″	1″	2″	2″—1″	1″	2″	2″—1″
Poetry												
1″ Stanza	43	61	67				36	39		44	45	
2″ "	49	62	77	+15	+11	+26	49	39	−3	55	54	0
Av.	46	61	72				42	39		49	49	
4 Grays												
1″ Trial	29	51	41				43	95		25	59	
2″ "	45	68	61	+22	−8	+13	39	61	+37	38	56	+26
Av.	37	59	51				41	78		31	57	
9 Tones												
1″ Trial	13	61	58				49	82		54	49	
2″ "	37	56	48	+34	−6	+27	51	91	+35	38	46	+1
Av.	25	59	53				50	85		46	47	
9 Grays												
1″ Trial	54	59	71				47	72		44	50	
2″ "	63	63	57	+2	+3	+6	42	68	+26	59	48	−2
Av.	59	61	64				44	70		51	49	
4 Tones												
1″ Trial	36	24	31				69	53		45	58	
2″ "	18	18	11	−6	0	−6	54	76	+3	38	69	+22
Av.	27	21	21				61	64		41	63	
Geom. Figs.												
5 Trials	44	53	47	+9	−6	−3	56	67	+11	22	22	0
9 Numbers	37	39	49	+2	+10	+12	37	48	+11	30	34	+4
Movement												
15 cm.	96	95	95	−1	0	−1	95	94	−1	94	96	+2
20 cm.	97	97	98	0	−1	+1	98	95	−3	94	93	−1
25 cm.	98	97	98	−1	+1	0	97	94	−3	94	95	+1

1″, 2″, and 3″ = First, second and third tests.
2″—1″ = Average of First test minus average of the second test.
\+ = Gain.
— = Loss.

TABLE 1 (Continued)

	E. M. C.			H. C. E.			A. R. F.			M. M. M.			M. L.		
	1"	2"	2"—1"	1"	2"	2"—1"	1"	2"	2"—1"	1"	2"	2"—1"	1"	2"	2"—1"
Poetry															
1" Stanza	41	56		54	65		61	70					89	97	
2" "	44	66	+19	84	73	00	48	59	+10				92	100	+8
Av.	42	61		69	69		54	64					90	98	
4 Grays															
1" Trial	10	83		24	98		25	73		15	48		19	69	
2" "	59	83	+48	63	100	+56	29	60	+39	33	46	+23	30	56	+39
Av.	34	82		43	99		27	66		24	47		24	63	
9 Tones															
1" Trial	27	47		23	94		31	43		30	36		28	38	
2" "	40	50	+15	52	99	+59	44	48	+8	50	34	+5	33	54	+15
Av.	38	48		37	96		37	45		40	35		31	46	
9 Grays															
1 Trial	39	64		26	93		53	70		42	31		40	68	
2" "	54	52	+12	53	98	564	23	71	+32	40	56	+2	52	67	+21
Av.	46	58		39	95		38	70		41	43		46	67	
4 Tones															
1" Trial	20	40		33	63		30	44		41	32		29	43	
2" "	19	29	+15	54	70	+23	20	46	+20	44	19	−16	23	45	+19
Av.	19	34		43	66		25	45		42	26		25	44	
Geom. Figs. 5 Trials	27	33	+6	45	80	+35	49	54	+4	44	62	+18	47	69	+22
9 Numbers	38	36	−2	43	46	+3	43	47	+4	29	38	+9	45	52	+7
Movement															
15 cm.	96	95	−1	98	96	−2	96	95	−1	97	95	−2	88	94	+6
20 cm.	98	96	−2	98	98	−0	95	96	+1	97	97	0	89	97	+8
25 cm.	97	97	0	97	96	−1	97	96	−1	97	93	−4	90	93	+3

Table II.

Results of First and Second Tests for Untrained Observers.

	M.C.			J.W.			M.D.F.			D.D.W.		
	1″	2″	2″—1″	1″	2″	2″—1″	1″	2″	2″—1″	1″	2″	2″—1″
Poetry*	48	34	00	65	65	0	70	63	+2	40	51	+5
	61	74					64	75		47	45	
	54	54					67	69		43	48	
Four Grays	21	37	+2	32	41	+7	25	28	+15	36	23	−7
	28	14		40	46		20	46		36	35	
	24	26		36	43		22	37		36	29	
Nine Tones	30	30	+5	33	55	+9	28	43	+14	24	28	+14
	29	38		63	60		39	51		18	42	
	29	34		48	57		33	47		21	35	
Nine Grays	19	43	+21	48	64	+11	40	44	0	22	36	+8
	33	51		65	72		38	34		25	30	
	26	47		57	68		39	39		24	32	
Four Tones	20	21	+2	37	31	−17	29	28	0	18	28	+6
	18	21		66	37		36	36		28	30	
	19	21		51	34		32	32		23	29	
Geom. Figs.	33	40	+7	44	64	+20	53	58	+5	31	31	00
Nine Numbers	39	31	−8	41	47	+4	42	44	+2	36	38	+2
Movement	94	94	0	88	97	+9	97	92	−5	96	96	0
	97	96	−1	92	97	−5	98	95	−4	95	96	+1
	98	95	−3	95	97	−2	97	95	−2	96	97	+1

* Notation same as in Table I.

consciously or subconsciously, of an individual way of picturing
the stimuli, sometimes also including the response, and which
5 consists of a particular form of imagery; fifth, the use of this
6 imagery until it becomes reflex; sixth, the appearance of rhythms
7 in the ability to hold the image-changes in attention; and seventh
the formation of associations between the giving of the stimulus
and the response, after the use of the imagery becomes reflex.

RESULTS OF THE TEST SERIES.

The results of the test series for all observers are shown on Charts I to XIII inclusive by the broken lines which run directly across the chart. The results are also shown numerically in Tables I to III. The ability in each of these tests is expressed in the per cent of correct responses, as in the training records. The tests are discussed in the order in which they were taken.

Trained Observers.

Poetry. The gain in the tests for poetry was not very great in the case of any observer. Various methods were used. A large number read the stanza over first. Some divided it into parts of two lines each, some of four lines each, some into two parts. More than half of the observers speak of using imagery in remembering. Several say that they pictured the lines on the page in relation to each other. Several divided the stanza into parts according to the pictures it contained. Many memorized by these images in the stanza, and then combined the images into a whole picture. Two observers say that the training series may have helped in securing improvement. One says that the training may have helped by emphasizing imagery, and the other says that it may have helped in dividing the stanza into parts.

The Four Grays. The gain in the four grays is often greater than the gain in the training; it is usually as great, seldom less. In the first test the methods used were—to catch alternate groups, to divide groups into two groups of two figures each, and to remember each group. F. S., especially, noticed a tendency to image the groups. His first impulse was to remember the

groups by a picture of the numbers of the grays or the grays themselves, but the time was too short to work out the picture for each group. Often observers tried two or three ways of remembering during the first test for the four grays. In the second test every trained observer but one says that the method developed in the training helped in securing a better record. Six of the trained observers say that they used the same imagery in the final test series that they used in the training series. There was more or less hesitancy in using the same imagery because of the difference in the stimuli of the training series and this test. The difficulty seems to depend upon the imagery. With G. C. F., whose imagery was visual-auditory for the tones of the training, there was great difficulty in using it with the grays. With D. S., whose imagery was visual, there was but a slight hesitancy in fitting the imagery into the responses for the grays. This was true also of H. C. E., A. R. F., M. M. M. and probably of E. M. C., for he says that the training helped him decidedly in the final tests, though he does not record a specific type of imagery. The same is true of M. L. F. S. says, in the introspection for the second test, that he remembered the groups by visual imagery with which he had no difficulty. He did not repeat the numbers as in the first test and as in the training series, but saw them in two groups of two in each group.

The Nine Tones. Four observers gained more in the nine tones than in the training series. Two made the greatest gain in the nine tones of any of the test experiments. The influence of the training therefore seems to be very strong. G. C. F. says that with him visual imagery is usually the strongest. Yet he made the greatest gain in the nine tones. His training imagery is auditory-visual, and he thinks that most of the gain in the second test for the nine tones is due to the influence of the training imagery. D. S. says that during the second test he was able to transfer his imagery system directly to this test by grouping the nine tones into fours. All the trained observers say that the training series helped in the second test. All except M. M. M. used the training imagery in the second test with the nine tones. M. M. M. divided the nine into numbers of three figures each, as 421, 343, 124. All had a different

way during the first test, and all found that the immediate succession of the second group of four tones after the first group led to confusion.

The Nine Grays. Three observers made a greater gain in the nine grays than in the training. One observer made his greatest gain in the nine grays. The introspections show the same characteristics regarding the influence of the training in the second test over the first. Two observers, G. C. F. and D. S., say that they find it easier to use their imagery with the tones than with the grays. But there is the same difficulty experienced in the immediate succession of the second group after the first, and the same change of method from the first to the second test. Under her introspection for this test M. L. descr bes her imagery:

> "Toward the last of this series I thought of a new method of getting these by fours. It was by sort of picturing them with braces connecting them, with a top brace being the first one thus:
>
> 1, 2, 3, 4, 1, 2, 3, 4, 1, 2, 3, 4,
>
> This would represent the combinations 1, 3, 2, 4, 2, 4, 1, 3, and 4, 2, 3, 1. This method helped me most when the grouping was something of the skipping order, and not 1, 2, 4, 3, where the numbers were right next to each other."

The Four Tones. One observer only, F. S., gained more in the four tones than in the training series, and no observer made the greatest gain in this test. As the name implies this test is most like the training series and therefore suggests that it should show the greatest gain if training is to influence tests similar to it. G. C. F. says that the failure to improve is due to the different method of response. Instead of responding by numbers the observers were instructed to respond by the syllables—Do, Me, Sol, Do-2. All observers say that there was a distinct tendency during the second test to use the methods and imagery of the training, but in the effort to make the syllable response, the tendencies developed in the training were broken up. Nearly all think that they could have made a better record if they could have responded by the use of numbers. The similarity to the

training series made the interference the more effective. M. M. M. says "the practice series was a hindrance here, because the numbers were so drilled into me that it was hard to change."

Geometrical Figures. Several observers think that the experience of the first test is sure to suggest methods for the second test. Motor imagery was strong with many observers for they moved the pencils over the paper while trying to retain the figures in memory between the trials of the test. The method most used was to remember the figures because of their similarity to letters of the alphabet. Some observers tried to remember the entire nine figures after a ten second exposure but most observers adopted the method of remembering two or three at each exposure. H. C. E. thinks that the training series helped here because he was able to group things together and think of two groups at once. M. M. M. knows of nothing in the training series that helped except that he was better able to concentrate attention on what he was doing. The other observers saw no connection between this test and the training series.

The Nine Numbers. F. S. made his greatest gain in this test. The common method here was to remember two or three of the first pairs, and to hold two or three of the last pairs because of their recency. No one recognized any way by which the training helped here.

Movement. The method employed by most observers is indicated by the introspection of D. S.: "The movements of the finger along the glass rod were always accompanied by an eye movement and a visual image of the distance traversed. It is evident that the estimate was made both by muscular and visual imagery." Not all observers recognized these factors but nearly all speak of visual and motor imagery.

Final Introspections. The opinion of the trained observers in regard to the factors that make for improvement and the relation of the training to the tests is shown by the following quotations from introspections written after the experiments were completed.

D. S.: "The following are the most important effects of the training series upon the test series:

1. The system of 'names.' The most important and effective factor.

2. Imagery was very prominent in the training and seemed to be more prominent in the second test than in the first.

3. Greater economy in mental effort and attention.

4. Development of ingenuity in devising methods. The method of 'names' was a very essential part of the improvement in the training series. Without it, I believe that I could not have reached the proficiency I did. In the last test, I felt that somehow I could better master the situation."

F. S.: "If I made any improvement in the last test, I think it was due to the following causes:

The idea that I was going to improve.

I found it easier to hold my attention on the work during the second test.

I felt a distinct sense of improvement in only one test, namely, the four grays. This was due to a change of method. I also have a feeling that I improved in the four tones. This may have been due to a general familiarity with the test. I was able to recall these tones with much less difficulty than the telephone tones. The gain in the four tones is due to the fact that I used the same method as in the training series. I had formed a certain habit of imagery which served me in this test."

E. M. C.: "There is a marked relation between the training series and the four grays, the nine tones, the nine grays, and the four tones, but apparently not much relation between the other experiments and the training series. Practically all the gain shown in the second test is due to the influence of the method used in the training series."

H. C. E.: "I think the thing that accounts for the improvement in the second test over the first is the system or method which was developed from the training series. In the last test series, I used the same method as in the training series, except that the system of imagery was changed slightly for the grays; that is, the grays appear in a vertical row just as for the tones, but instead of each being represented by a disk of different size they are now the same size and have the respective brightnesses of the four grays. I think it would make no difference what sort of tests I might be given where these four numbers were used; I could do as well as with the tones and grays."

Untrained Observers.

The methods used by the untrained observers will not be described unless they differ, in particular tests, from the methods used by the trained observers.

The Four Grays. Three of the four observers note a tendency to visualize the four grays. M. C. says, "The visual impressions seemed rather strong during the first test." In the second test, however, she abandoned the attempt to image. M. D. F. speaks of the use of an imagery he had adopted in some of the first tests. In the second test D. D. W. says, "I

tried to remember the grays as a row of figures; also to get a mental picture of them but the time was too short."

The Nine Tones. M. C. says that she saw the figures in a "group imagery" and retained them in that way. M. D. F. tried to repeat the numbers as tones, that is, high tones to low tones in each group, or low tones to high in each group of four. The four tones gave him an image of a board with four keys but he is not able to state the form of the keys or of the board, but tried to remember how the place or point suggested by each tone would skip around. In the introspection for the second test, he says:

> "I can see fairly distinctly before me a key-board, and the tones go up and down. Four is at the top, and one is at the bottom, and I simply let the tone suggest the position, and when it comes time to respond, the image of the key-board returns. This test seems easier for me than the other experiments and also easier than the first test of this experiment, that is, it appears that the key-board helps me to remember."

With D. D. W. and J. W., a number of methods were tried during the first test. Both adopted the method of grouping by fours during the second test.

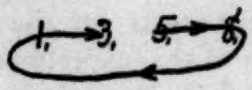
The Nine Grays. In the first test, M. C. tried to picture the numbers as they were given and also to note how often one of any kind was repeated. In the second test he tried to remember the nine grays in groups of fours, but this time they appeared not in imagery of figures, but as a picture of the dots. This aided materially in retaining the groups. J. W. says that he tried visualizing, but it was a complete failure because of lack of time to recall the picture in detail. M. D. F. says:

> "Each color as it appeared suggested a number, and I had no trouble in classifying the colors. Nine, however, was too great a number for me to retain at once. Some groups I found much easier than others, for instance, 4, 1, 2, 3, and 3, 2, 1, 4, etc., appeared arranged in a vertical column, and attention skips from the four, which is always at the top, to the other numbers in the order in which they were given, somewhat as follows:"

4	4
3	3
2	2
1	1

M. D. F. used the same method in the second test. D. D. W. said that he tried to remember all the numbers of the group, but the nine seconds interval was too short, to form a distinct image of either the grays or the numbers they represent.

The Four Tones. M. C. thinks that the imagery which was used in the nine grays assisted here, but says that the different response interfered. In the first test J. W. tried to form an auditory image of the four tones but failed. He complains of an annoying fusion of the tones, a sort of harmony, which prevented him from remembering. With M. D. F. each note suggested a position on the piano, as 1, 3, 5, 8, and he remembered the order of the tones in that position. For instance, 5, 8, 1, 3, would be like this:



As long as he kept his mind concentrated on the above form he could remember, but it was very easy to allow the second group to confuse the one already received.

The final introspection for M. C. describes the form of imagery used and therefore it is quoted entire:

"I felt that I did better in the last end series than in the first, because I understood what I was supposed to do and was not confused. The only thing I know that helped me was the picture of the four dots, the dots appearing in a horizontal line,—

It seemed to me that if I could close my eyes during the test for the four and the nine grays, and not have the other group follow so closely, I could get them all correct. I used this imagery in my last tests for the four groups, but it seemed much easier with the grays than with the four or nine tones. This method did not help me in the other four tests. They seemed independent, and concentration in the first test did not help me in any of these four tests—geometrical figures, nine numbers, poetry, and movement. My greatest hindrance was that I often felt that I was not getting them right, and that confused me so that I could not concentrate my attention."

COMPARISON OF RESULTS.

Definition of Transference. It is first necessary for us to seek an understanding of what is meant by transference. We may mean by transference, the ability to use in one act the elements involved in another act. If we mean by transference that the training one receives in using a number of elements in one act is transferred to another act in which these elements do not occur, then the phrase "spread of training" would describe our meaning more accurately. In the sense of "spread of training," we can hardly say that there is "transference." A technical meaning of transference is answered only by the first definition. Both conceptions are involved in the present experiments.

If, as in the first definition, transference means simply the use of identical elements in different tasks, then the analysis of the conditions must be somewhat as follows: Let us suppose that any act is composed of the elements a, b, c, d, e; another act of the elements l, m, n, o, p. The two acts would be represented by two sets of elements, totally different, in which case there could be no such thing as transference. If, however, we examine an act composed of the elements a, b, c, d, e, and a second act composed of the elements e, f, g, h, i, where the element e appears in both acts, we would have the possibility of transference as far as the element "e" is concerned. If we had an act composed of the elements a, b, c, d, e, and another composed of the elements b, c, d, e, we would have a much stronger possibility of transference since all the elements except a in the first act are identical.

But, in the case of "spread of training" if we had an act composed of the elements a, b, c, d, e, and another act composed of the elements f, g, h, i, j, then the influence of training in the first act upon the elements composing the second act, will be measured by the amount of relationship between the elements of the two acts, that is, it will be measured by the amount of subconscious and actively conscious connection made between the elements by the observer. The application of the principle of "spread of training" can be made only to such cases where the elements concerned in the two acts are not identical, but related.

Of course, it were to beg the question if we used the phrase "natural connection," or the phrase "a natural relationship." The experimental difficulty involved is to determine which elements of experience are identical, which are related and which unrelated. The answer differs with individuals. Things related in one mind are not necessarily related in another mind. The relation between the things in experience depends, therefore, upon the relation which each person establishes in his own life—whether the frequent occurrence together of elements has built them into a purely automatic relation, or a connection is worked out by logical consciousness. At any stage of life an individual has a lot of relations which are automatic and another lot in which he has traced out a conscious relationship. We are often surprised by the discovery of a relation where we thought none existed. On the other hand we must guard against the conception that any mental components of a complex act enter into another complex act without being modified.

Relation of the Gain by the Trained to the Gain by the Untrained.

The following lists show the order of the tests arranged according to the greatest improvement for the trained and untrained observers:

Trained Observers.	*Untrained Observers.*
Four Grays	Nine Tones
Nine Tones	Nine Grays
Nine Grays	Geometrical Figures
Geometrical Figures	Four Grays
Four Tones	Poetry
Poetry	Nine Numbers
Nine Numbers	Movement
Movement	Four Tones

The rank of these tests, as shown by the greatest gain for each observer, is indicated in Table III. The four grays, with seven observers, show the greatest gain, and second greatest gain with two observers. The nine tones received first rank

with three observers, and second rank with five observers. The nine grays received first rank with two observers and second with two. The four tones received no first or second rank.

TABLE III.

Comparison of Results by Gain or Loss.

	G. C. F.	D. S.	F. S.	E. M. C.	H. C. E.	A. R. F.	M. M. M.	M. L.	M. C.	J. W.	M. B. F.	D. D. W.	Average Trained	Average Untrained	Difference between Trained and Untrained.
Training Series	32 (24)	25	−9	41	27	29	17	[9]					21		
Four Grays	22	*37*	*26*	*48*	56	*39*	*23*	*39*	2	7	*15*	−7	36	4	32
Nine Tones	*34*	35	1	15	*59*	8	5	15	5	9	14	*14*	22	11	10
Nine Grays	2	26	−2	12	56	32	2	21	*21*	*11*	0	8	19	10	9
Four Tones	−6	3	22	15	23	20	−16	19	2	−17	0	6	10	−2	12
Average	13	25	12	23	49	25	4	24	8	11	15	5	22	6	16
Difference between training gain and test of greatest gain	2	12	35	7	32	10	6	30					18		
Geom. Figs.	9	*11*	0	6	*35*	4	*18*	*22*	*7*	*20*	*5*	0	13	8	5
Nine Numbers	2	*11*	*4*	−2	3	4	9	7	−8	4	2	2	4	0	4
Movement	−1	−2	1	−1	−1	−1	−3	6	−2	1	−4	1	0	−1	−1
Poetry	*15*	−3	0	*19*	0	*10*		8	0	0	2	*5*	7	2	5
Average	6	4	1	6	10	5	6	11	0	6	1	2	6	3	3

All figures represent differences between first and last tests.
The record of the test which shows the greatest gain is printed in italics.
Minus sign means loss.

In Table III, the tests are ranked by similarity and by dissimilarity to training series. In the ranking for first and second places geometrical figures received first rank with six observers and second with three. The nine numbers received first rank with two observers, and poetry received first rank with four observers. Movement did not receive first, second or third rank with any observer.

The amount of gain of the trained over the untrained is shown by the amounts recorded in the last column of Table III. This column shows that for the four grays there was a difference between the results for the trained and the untrained of 32 per cent, or a gain nine times greater in the trained than in the untrained; for the nine tones, a difference of 10 per cent, or a gain twice as great for the trained as for the untrained; for the nine grays a difference of 9 per cent or a gain twice as great; for the four tones, a difference of 12 per cent; for geometrical figures, a difference of 5 per cent; for the nine numbers, a gain of 4 per cent; for the movement a difference of – 1 per cent; and for poetry a difference of 5 per cent.

The difference of the gain of the trained over the untrained in tests intentionally similar to the training series is 16 per cent. The corresponding difference between the trained and the untrained for tests intentionally dissimilar is 3 per cent. The gain of the trained in tests similar to the training is three and one-half times as great as their gain in tests dissimilar. The gain of the untrained in tests similar to the training is twice as great as their gain in tests dissimilar.

Let us now analyze, if possible, the influence of the training series upon each of these tests.

The four grays differed from the training series in but one factor, that is, grays instead of tones were used for stimuli. A reference to the table shows that the average gain for all observers in the four grays over the training series, is about 15 per cent. This is true of every observer except one.

The test showing the second greatest gain was the nine tones. Here the test differed from the training series in method, but was identical in content. In estimating the influence of the training upon test experiments in tones the first test may be regarded as the first practice series. This would tend to lift the recorded results for the first training above the first record for the tests in tones. This, in turn, would tend to increase the gain of the test over that of the training series. On the part of five of the observers there was a gain in the nine tones greater than the gain in training. The average shows a difference of 1 per cent in favor of the nine tones.

The nine grays show the third greatest gain. In the case of five observers the gain is greater in the nine grays than in the training. The difference between the averages of the two for all observers is greater by 2 per cent for the training than for the nine grays. The nine grays differ from the training series in both content and method.

The four tones differ from the training series in the use of pitch differences, instead of differences of intensity of the same tone, and in the method of response. The results show a gain in the training series greater than in the four tones, except for two observers. In reality there is a gain in the tones greater than in the training in the case of one observer only, for M. L. trained but two days. F. S., the other observer, made no gain in his training, but there was a gain of 22 per cent in the four tones. It seems that of those who gained in training and trained the full time, there was in no case a gain in the four tones equal to the gain in the training. In two cases there was a loss in the four tones. For all observers the average shows that the gain in the training was twice as great as in the tones. It is to be pointed out that this test is very similar to the training series. The different response required was planned because it was thought that it would be more familiar to the observers than the number response. As already mentioned, however, the introspections show that it was the different nature of the response that accounted for the failure to gain more in the four tones; several observers speak of this as a hindrance to correct responses.

Table III, shows also the difference between the gain in the training series and the gain in the test making the greatest gain. Every observer made a greater gain in these latter than in the training series.

The test showing the greatest gain for trained observers in the class of dissimilar tests was geometrical figures. In the case of two observers (including the observer trained for two days only) there was a greater gain in this test than in the training series for the same observers. This is the test among those unlike the training series in which the untrained observers make the greatest gain. This test also shows a greater gain in the

case of trained observers than does the test for the four tones. But for the other dissimilar tests there is so small an improvement that the trained observers gain on the average three and one-half times as much in the similar tests as in the dissimilar ones.

Table III indicates great variations in the amount of improvement made by different observers in the different tests. It might be expected, judging from the way the experiments were planned, that it would be easy to subtract the amount of improvement in the several tests from the improvement in the training series, and thus arrive at a direct estimate of the influence of the training upon that particular test. This, however, is not an easy matter. Individual differences and factors beyond those brought out in the numerical results enter to complicate the estimates. It is only possible to make such an analysis when the introspective evidence is sufficiently full and accurate to enumerate and define all the factors involved.

Three significant features have thus been noted in the above table; first, the difference between the improvement of the trained over the untrained; second, the difference of improvement in the tests similar to the training series in trained observers over their improvement in tests dissimilar; and third, the greater amount of improvement in the tests than in the training.

The Factors in Improvement and Transference.

The things most commonly mentioned by observers as contributing to improvement and transference have already been enumerated. It remains to point out some of the considerations bearing upon the interpretation of these factors.

Imagery. No suggestions were given observers regarding imagery. Indeed, it was not until the experiments were completed and work on results began that the uniformity of the testimony of observers concerning it was realized.

Eight of the twelve observers, all but two of the eight trained and two of the untrained, record a specific type of imagery. The two trained observers, F. S. and E. M. C., who did not do so, show by the language of their introspections that they

used imagery, but did not recognize a particular form. Of the untrained observers, two recorded the development of a complete imagery system, while with the other two there was a strong tendency to develop an imagery. As is shown by their introspections, in the case of observers who took only the test series, it is evident that the brevity of the tests together with the rapidity of change, would not permit the development of imagery with such facility as in the training series. Nevertheless two of the untrained observers, M. C., and M. D. F., developed a specific, well recognized imagery.

The fact that the stimulus of the training series was sound would lead us to expect auditory imagery. However, reference to the introspections already quoted will sufficiently indicate that there are three main types of imagery represented among the observers. G. C. F.'s may be called a visual-auditory type; that of D. S. and M. C., a purely visual type, and that of M. M. M., A. R. F., H. C. E., M. L., and M. D. F., a visual-motor type.

Everyone who does any act like this memorizing has a characteristic method. The evidence derived from these experiments indicates that the essential element in method is imagery. Having once selected, consciously or unconsciously, an imagery, improvement seems to depend upon the fidelity of the observer to that imagery. Improvement depends also upon the fitness or adequacy of the imagery to do the thing for which it was adopted.

Here

Whether each has an imagery for each separate act, whether each has a great many forms of imagery, corresponding perhaps to the customary things of life, or whether we have a few forms of imagery which we use for many different things, are interesting questions. If we do not have an imagery for each act, then the question of the use of imagery in different acts is just the one we are seeking to answer in regard to "transference" or "spread of training" by these experiments.

If an imagery is selected which is complicated, such as that of A. R. F., the observer is doing no other than selecting a complex method, which requires longer use to secure accuracy and speed. Or, if one selects an imagery which is not adequate to

the task as a whole, but serves for part only, such as an imagery for certain groups of sounds in the practice series, illustrated possibly by the imagery of D. S., then the observer must adopt a double or even a manifold system of imagery, and improvement in speed and accuracy would seem slower. Also, if one should change his type of imagery, it would lead to a lack of improvement or to fluctuation in improvement. This is a possible explanation of the failure of F. S. to improve.

For a short practice series, it would seem better to adopt a method or type of imagery as soon as possible, and, even though it is found to be cumbersome, remain faithful to it; for tasks long continued or to be oft repeated, the sooner one selects the best imagery, the better for the final outcome. Native ability finds its field in the readiness with which one selects an imagery adequate to secure the accuracy and speed demanded by a skillful performance of the task.

The prominence thus given to imagery as the essential characteristic of method has been pointed out before. Binet, in his "Psychology of Reasoning," insists upon imagery as the essential factor in all mental operations. Nearly every research in imagery since then has indicated something of the large place which imagery occupies in mental life. Coover and Angell have shown the value of the "careful elaboration of the plan of work, the actual working out of the method in the form of detailed introspections, and the searching and thorough analysis of results in experiments of this kind." Their research, however, not only fails to bring out the fact of an individual imagery, but even seems to seek to eliminate imagery altogether as a factor in improvement in training. Its presence, however, seems to be indicated in some of the introspections quoted.

"Am able to abstract from visual imagery of the apparatus entirely, and yet refer sounds to external stimuli. This seems to take the least effort, and is most satisfactory,"

They say—"The introspections indicate that the discrimination processes were accompanied by much imagery from other domains of sense, which in some cases determined the judgment. This imagery was largely kinesthetic and visual." "One reagent seemed to compare the intensities of bodily reactions to the sound stimuli themselves or to imagery called up by the stimuli, e. g., the 'flash of a bicycle lamp.'"

Still they say explicitly—"Many introspections * * * near the end of training were, 'No imagery.'"[1]

The relation of type of imagery to "transference" or "spread of training" is indicated in part by the results. In the case of G. C. F., the visual-auditory imagery used was that of a localization in space of the four tones. When in the test experiments, the four tones were changed to grays, there was a strong tendency to remember the grays in the same manner as the tones were remembered, because the stimulus rhythm and the method of response were the same. But the grays refused to take the position in space that had been customary in the case of the tones. An improvement was made between the tests for grays, but this tendency to use the practice imagery had to be overcome. The more thoroughly he was trained in the use of his imagery the less able was he to make a good record in the tests where he found a tendency to use it, but to which it seemed inapplicable. He gained about 20 per cent during his second practice period, but the results of the third test series shows not only no gain over the second test results but an actual loss of 8 per cent in the four grays, and of 6 per cent in the nine tones. These are, however, the tests in which he had made the greatest gain between the first and second tests. He gained but 3 per cent in the nine grays in the third test, and he made no gain in the four tones. His tendency to gain is shown by the gains of 11 per cent in poetry, 10 per cent in nine numbers, and 6 per cent in geometrical figures.

Nearly every observer, especially those who developed a clear imagery, was troubled with the same difficulty in the case of the four tones, for here the change in the response interfered with the use of the practice imagery.

On the other hand with D. S., H. C. E., A. R. F., M. M. M., M. L., M. C., and M. D. F., the type of imagery developed was as easily used with the grays as with the tones. These observers illustrate the benefits of making an imagery capable of being used in several acts thoroughly automatic as quickly as possible. With these observers, the longer

[1] Coover and Angell, *op. cit.*

they were trained, the easier it became to use the automatic imagery in the tests. So strong did this connection seem to H. C. E., that he said: "I think it would make no difference what sort of test I might be given where these four numbers were used; I could do equally well as with the tones and grays." The longer the observer was trained, therefore, the more non-essentials were cast aside, while the few essentials became habitual. When attention was long confined to the essentials, each element among them became welded into the imagery system.

Now, if a task differing in one essential only, from the trained one is given, the whole system feels the shock of the change in a vital part, until the adjustment is made. If the new task differed in two or three points, the shock is still greater. If the task was so different that the observer recognized no similarity, that is, if for him there was no way of applying his system of imagery, or if the imagery did not apply itself, then a new system of imagery was built up for the new task. It would seem, then, that the best time to make transfers of training in tasks which we recognize as dissimilar, is in the moments of beginning a new task, because the non-essentials which we use at first may be the essential ones in the second task. Thus, there may be advantages in learning several acts at about the same time.

Cases in which the amount of improvement in the test is greater than the amount of improvement in the training, are explained in part by the nature of the imagery used by the observers; the imagery used by the majority of observers was more readily applied to the tests than to the training. Such imagery as that of H. C. E., A. R. F., M. M. M., M. L., M. C., and M. D. F., supports such a view. The question of transference, then, becomes in very large part, a question of the nature of the imagery employed in the practiced task.

The significance of practice in the first test must be estimated here. Tables I and II show the difference between the first and second trial of each test, for both the first and second tests. It will be noticed that the gain between the first and second trials of the first test is often greater than the gain between the two tests. This is in accord with the well known fact that practice shows the greatest gain at the beginning of a training series.

The influence of one end test upon another is, therefore, the more serious in the "before" tests; and the effect of these tests upon the beginning of the training series may, in some cases, amount to more than the effect of a day of training in the training series.

The relation of improvement to one's ideas of improvement has often been raised as an experimental question in psychology. Many experiments have shown how often results differ from the feeling regarding improvement. It seems probable, from this series of experiments, that the feeling of improvement or the lack of it, is more or less closely connected with familiarity or lack of familiarity with imagery. All observers in this series of experiments were kept ignorant of results, but were asked to note in their introspections their own feelings regarding improvement. It often occurred that the feeling and the fact coincided. This seems to be more uniformly true in the case of those who developed a recognized form of imagery. It seems to be more often true in the case of those who did not recognize an imagery, and of those who had not yet recognized their imagery, that the fact did not correspond to the feeling.

The factor of attention and its control seems to be an important one in improvement and transference. In the opinion of observers it ranks next after imagery. Introspections at the beginning of the tests, and early in the training, show that observers recognize attention or the lack of its control as an important element in selecting the essentials from the non-essentials. Many speak of the rapid fluctuations of attention at this time. Observers who had a vivid imagery, speak of the fluctuations of attention in the use of the imagery; later in training, when the use of the imagery has become automatic, they say that co ntrol of attention seems to be the chief factor in rapid improvement, and the lack of it, the cause of error. Nearly every observer who seemed to approach the limit of his ability in training, testifies that the slightest fluctuation of attention produces a change in the results. In early training, therefore, attention seems to be drawn easily to the new conditions of work, i. e., to non-essentials. In improvement during practice, attention is more and more given to the central element concerned,

i. e., to the imagery which the observer uses. Toward the limits of training, attention may be permitted to run off on associations for the automatism of the imagery permits extra time between the stimulus and the response. When observers are making every effort to miss no stimulus or response, slight disturbances, such as slight changes in the stimulus, or noises from without, break the rhythm and produce rapid changes in attention. Practice curves of these observers, if plotted with regard to the grouping of the fours, where there is no change of method, are an excellent representation of the normal fluctuations of attention.

Association is another factor in training and transference. Most observers say that at the beginning of training and in the first test there is no time to form other associations than those among the elements concerned, but toward the close of training, nearly all speak of lapses of attention, due to associations with outside things formed in the interval between the stimulus and the response, or between the response and the stimulus. The relation between the training series and the test series may be called association, but it is better defined from the standpoint of imagery.

Automatisms have already been mentioned several times in connection with training and transference. It is inevitable that they should be formed in any process of training. The rapidity with which they are produced depends directly upon the fidelity of the observer to the imagery adopted, and upon the simplicity or complexity of the imagery to the observer. For example, H. C. E. adopted an imagery the first day of training and used it throughout training. His imagery was to him easy and readily used, and became automatic very quickly and thoroughly. A. R. F. did not recognize an imagery early and, when he did recognize it, it seemed to him complex and difficult of use. His imagery became automatic slowly, and before it became very thoroughly so, the training was over.

The relation of automatisms to the final tests is one of assistance, or of interference. The more automatic an act becomes, the less likely are its elements to be transferred to unlike elements. If it can be used, all goes smoothly. But if the task

is sufficiently different in content or method, for the observer, to awaken conscious efforts to use it in the new tasks, then automatism becomes a hindrance. In improvement in training, therefore, the more quickly automatisms may be cultivated the better. In transference, the cultivation of automatisms may be either a help or a hindrance according to the nature of the imagery of the observer.

GENERAL CONCLUSIONS.

The original research by Professor James (Psych., Vol. 1, p. 667), which served as the starting point for the investigations, contains this sentence, "All improvement of memory consists in the improvement of one's habitual methods of recording facts." Several experimenters have interpreted their facts for or against James' conclusions as seemed evident to them. The fact is, however, that many researches interpreted adversely are capable of interpretation to support his contention. A research which the writer carried on with Professor Gilbert, published in the University of Iowa Studies in Psychology, Vol. I, on "Practice in Reaction and Discrimination" left a distinct impression in the writer's mind that Professor James was wrong. The evidence of that same research seems now to be capable of an interpretation in support of Professor James as otherwise. Among the researches which have been interpreted as against James' conclusions are those of Judd (Psy. Rev., Vol. IX, pp. 27 to 39); several researches on cross-education, such as those of Scripture, Smith and Brown (Yale Studies Vol. II), Davis (Yale Studies, Vols. VI and VIII), Ebert and Meumann (Arch. f. d. ges. Psy., Bd. 4). The researches which take the ground apparently in support of Professor James are those of Thorndike and Woodworth (Psy. Rev., Vol. VIII), Bair (Psy. Rev., Mono. Sup. No. 19), Coover and Angell (Am. Jour. of Psy., XVIII, p. 328). A distinct effort to analyze the elements concerned in improvement in practice and in transference has characterized the later researches. As typical of this tendency, we may quote the researches of Thorndike and Woodworth, and of Coover and Angell. Thorndike and Woodworth say that after

practicing with rectangles 10 to 100 sq. cm., observers learn that one has a tendency to over-estimate all areas and consciously make a discount for this tendency, no matter how different other sizes or shapes of surfaces used in tests may be; also to look for the variations or the exceptional occurrences among the elements involved in training and in tests; third, learning to estimate in comparison with a mental standard, rather than an objective standard. This analysis of factors involved has a bearing only upon the tests carried on by these experimenters. They simply point out what seem to them to be the elements in their set of experiments.

Coover and Angell give a more translatable list of elements that seem to them concerned in improvement and transference:

> "We find, therefore, causes of the tranference of facility to be: (a) the formation of a habit of reacting directly to a stimulus without useless kinesthetic, acoustic, and motor accompaniments of recognition, which results in (b) an equitable distribution of attention to the various possible reactions so as to be about equally prepared for all; and (c) the consequent power of concentrating the attention throughout the whole series without distraction."

The elements that appear on the surface in our experiments are, while in the main in support of the analysis given by Thorndike and Woodworth, and Coover and Angell, contain elements both somewhat at variance with, and in addition to, those discovered in these researches. If, in the following from Coover and Angell: "Improvement seems to consist of divesting the essential process of the unessential factors, freeing judgments from illusions, to which the unnecessary and often fantastic imagery gives rise, and of obtaining a uniform state of attention which is less than a maximum," and "useless kinesthetic, acoustic and motor accompaniments of recognition," by "fantastic imagery" is meant such imagery as appears in our experiments or if it means such imagery as one of Coover and Angell's observers mentions, when he "seemed to compare the intensities of bodily reactions to the sound stimuli themselves or to imagery called up by the stimuli, e. g., the "flash of a bicycle lamp," then we must regard our results as distinctly divergent. Such imagery is an essential factor, if not the most essential factor in training and transference. With Coover and Angell's

general conclusion regarding the factors common in cases of training of dissimilar stimuli; i. e., "the habit of stripping the essential process of unnecessary and complicating accessories," we are in agreement.

In regard to the experiments of Thorndike and Woodworth, the difference between their conclusions and the conclusions of this series may be pointed out as follows: "After one gets mental standards of the areas, he judges more accurately, if he pays no attention whatever to objective standards." If Thorndike and Woodworth mean by this the same condition of imagery as developed in our experiments, which we imagine is possible, that is one point of agreement.

"Improvement in any single mental function need not improve the ability in functions commonly called by the same name. It may injure it." With this our conclusions also agree. Some definition, however, as Thorndike admits, needs to be made of the phrase "single mental function."

"Improvement in any single mental function rarely brings about equal improvement in any other function, no matter how similar, for the working of every mental function-group is conditioned by the nature of the data in each particular case." The results of our experiments do not support the statement contained in this sentence, especially in the first half of it. Improvement in many cases is absolutely greater in amount in the tests than in the training. The truth of the latter part of the quotation is verified in our experiments if the word "imagery" may be substituted for the word "data."

"The very slight amount of variation in the nature of the data necessary to affect the efficiency of a function-group makes it fair to infer that no change in the data, however slight, is without effect on the function." This our results corroborate.

"The loss in the efficiency of a function trained with certain data, as we pass to data more and more unlike the first, makes it fair to infer that there is always a point where the loss is complete, a point beyond which the influence of the training has not extended." Again our results corroborate.

"The rapidity of this loss, that is, its amount in the case of

data very similar to the data on which the function was trained, makes it fair to infer that this point is nearer than has been supposed. Again our results corroborate.

In the light of results here secured, we would change the following statement: "The general consideration of the cases of retention or of loss of practice effect seems to make it likely that spread of training occurs only where identical elements are concerned in the influencing and influenced function," to read—spread of practice occurs only where an imagery develops capable of being used by the individual observer in both training and test fields.

Our results do not corroborate the following statement from Coover and Angell, p. 339, as far as the freeing from any system is concerned:

> "At the beginning of training, they matched the color of the cards with the labels on the compartments; then to increase speed a system of mnemonics is employed, designed to form associations in the mind between a compartment and its color; this system then goes through a process of mutation,—becomes abbreviated, changed in part, supplemented, or is superseded by another; finally, through repetition, reactions to particular compartments become coordinated with their respective colors and are made directly—free from any 'system' except in rare cases."

The evidence from the introspections of all of our observers shows that there is no tendency to do away with the imagery or to free from the imagery system. Such cases as D. S., who had been trained by a long series of reactions practically identical with those in which he is trained here, and of G. C. F., who was trained for two months in the practice reactions used in this experiment, reactions which were selected for the intensity of application required in improvement and because of a possibility of reaching the limit of training for different observers within the practice period—such cases do not show any tendency to abandon the system. In this, therefore, our results do not agree with those of Coover and Angell.

With the statement of Professor James our results are in accord inasmuch as all the factors we have discovered have to do with methods.

There are two factors then, which we are seeking to analyze; first, to determine the factors that make for improvement; and

second, to determine the factors that make for spread of training or transference of training. If the problem were attacked from the standpoint of numerical results only, the analysis into elements would be most confusing.

SUMMARY OF CONCLUSIONS.

Some elements concerned in *improvement* and *transference* have been enumerated. Of these the central or most essential element is individual imagery.

Improvement seems to depend upon the consistent use of some form of imagery, whether it is the most advantageous form or not.

Imagery may be sub-consciously developed, but if it comes to be consciously recognized the improvement is more rapid. The rate of improvement seems to depend directly upon the conscious recognition of the imagery, and upon attention to its use.

A change of imagery during practice increases the rapidity of the improvement if a better form is adopted and adhered to. It may prevent improvement if a change of imagery is frequent, or if a less adequate form is adopted.

Individual differences are clearly shown in different types of imagery by the rapidity with which the imagery develops, and by the clearness or definiteness of the imagery.

The habit of guessing interferes with the formation of imagery and therefore, results in lack of improvement.

Transference may be divided into two kinds. It is either the use of identical elements in different tasks, or it is of the nature of "spread of training." The evidence of these experiments is in favor of the use of identical elements, or at least in favor of a limited spread of training. We are able to say that transference depends upon the nature of the imagery employed in practice, rather than upon any other factor. Whenever the training has become automatic and the difference between the training and the test consists of a few elements, these different elements serve as a hindrance only. We have then something of the nature of spread of training. If the difference is so very

slight that the elements are practically identical, as between the four tones of the training series and the four grays, there is little difference between the gain of the training and the test series. We have here something of the nature of transference, though transference as we have defined it, demands a complete identity between the elements of the acts. When the acts are made up of quite different elements, there is a distinct breaking up of the habit of responding, by the intrusion of the different elements, which raises the whole act into active consciousness so that the transfer of elements from one act to another act, other than the identical ones, is a conscious transference. It seems, therefore, that a conscious effort to use the elements of training in a different task, assists in making the transfer.

Factors that lead to improvement in the training do not necessarily lead to improvement in the tests; they may hinder it. The nature of the imagery, and the training in it seems to determine this. If, in the mind of the observer, the imagery is capable of adjustment to different tasks, it can be used in both improvement and transference, for the elements of the training act are thereby made the same as those of the test act. If it is adapted, in the mind of the observer, to the training task only, it may assist in improvement but it may interfere with transference.

Native ability appears to have abundant opportunity in the recognition of similarity or difference in the capability of the imagery for use in various tasks.[1]

[1] "Images, along with sensations, constitute the material of all intellectual operations: memory, reasoning, imagination, are acts which consist, in an ultimate analysis, of grouping and co-ordinating images, in apprehending the relations already formed between them, and in reuniting them into new relations." BINET, Psychology of Reasoning.

"Just as the body is a polypus of cells, the mind is a polypus of images." Taine, "On Intelligence."

THE EFFECT OF PRACTICE ON NORMAL ILLUSIONS

BY

C. E. SEASHORE, EDWARD A. CARTER, EVA CRANE FARNUM, AND RAYMOND W. SIES.

The following experiments are an outgrowth from experiments made by one of the writers in 1894 on the persistence of the size-weight illusion.[1] At the time that those experiments were made illusions of the kind were comparatively unknown and it so happened that the four observers experimented upon were all completely ignorant of the illusion. They had, however, been selected as the most intelligent and cautious observers among the advanced students. The experiments ran through twenty days, a half hour each day, with each observer. All exhibited a strong normal illusion, somewhat larger than that found as the average for ten other observers of the same type. And all came out alike in showing the surprising conclusion that the twenty days of practice revealed no tendency to decrease the illusion. At the conclusion of these experiments the results and the significance of the illusion were explained in detail to each observer and another test was made to determine the effect of knowledge of the illusion. Again the results were alike for all the observers showing that the knowledge of the illusion immediately decreased it by nearly one-half of its original force.

The measurements on the persistence of the illusion of the vertical made by Dr. Williams in 1901 also belong to this series of studies.[2] She had three observers and trained them for ten days each making one hundred trials each day for each observer.

[1] Seashore, "Measurements of Illusions and Hallucinations in Normal Life," Yale Studies in Psychology, III, 5–9.

[2] Williams, "Normal Illusions in Representative Geometrical Forms," Univ. of Iowa, Studies in Psychology, III, 108–116.

The first of these observers had no knowledge of the illusion, the second had partial knowledge, and the third (C. E. S. of the present series) had full knowledge of it. The first two observers exhibited an abnormally large illusion, both averaging 21%; the third averaged 6%. The following conclusions were drawn:

> "The illusion fluctuates in strength from day to day, especially for the observers who are aware of its existence.
>
> The practice gained in one thousand trials does not decrease the force of the illusion of the vertical for the line: this is equally true for those observers who know of the illusion and those who do not know of it.
>
> For one observer, who has had extensive experience in the observation of this illusion for years, the illusion still has a normal force."

The results of the investigation first mentioned were remarkable in that they demonstrated the normal persistence of the illusion so long as the observer has no knowledge of it. The second investigation resulted in another surprise in that the illusion persisted also in those observers who had knowledge of it. In the meantime Judd[1] reported experiments on the Mueller-Lyer illusion showing that the illusion disappeared with practice, and without leaving any conscious trace of the process of correction. This became a further stimulus to a search into the conditions which determine the effect of practice, especially with reference to different types of illusion, different degrees of knowledge, different capacities in critical attitudes, duration of practice, and the consciousness of gain.

To contribute toward the solution of such problems the following four studies have been undertaken in coöperation, each devoted to one type of illusion. The first named writer is responsible for the general plan and supervision of the experiments and has written this article as a synopsis of the four independent reports written by the respective experimenters.

[1] Judd, "Practice and its Effects upon the Perception of Illusions," Psychological Review, IX, 27–39.

I. THE ILLUSIONS IN THE LENGTH OF A CYLINDER.

Measurements by Edward A. Carter.

A cylinder looks to be longer than it really is. This overestimation has been analyzed by Williams[1] into several constituent illusions. Thus, the dimensions of a surface are overestimated when compared with a line; the surface of a solid is overestimated when compared with a plane surface, and, there is some peculiarity about the cylinder which leads to a further overestimation of its length. These three errors Williams has called respectively the area illusion, the volume illusion, and the illusion of cylinder length. When a cylinder is placed in a vertical position, the illusion of the vertical also enters.

Four observers went through similar training series on this illusion in its gross form, without any attempt to isolate the constituent elements.

As illusion objects we used three black metal cylinders, each 114 mm. in diameter and, respectively, 109 mm., 114 mm., and 119 mm. in length. The object in using more than one cylinder was to prevent the forming of any absolute standard of the length. The practice consisted in repeated measurements which were made in terms of a straight line by the method of production with the apparatus described by Williams.

This apparatus consisted essentially of a frame one meter square placed in an erect position and covered with manilla cardboard near the center of which a 2 mm. watch spring protruded through a slit and lay flush against the surface. The length of the exposed part of this spring was regulated by cords in the hands of the observer and a permanent millimeter scale on the back enabled the experimenter to record each setting. A similar frame having an inconspicuous wire support for the cylinder near its center was placed by the side of this. The frames were placed edge to edge and so turned that their centers would be on a level with the eyes of the observer and at right angles to the line of regard of the observer when seated at a distance of one meter.

[1] Williams, M. C., "Normal Illusions in Representative Geometrical Forms," Univ. of Iowa, Studies in Psychology, III, 38–139.

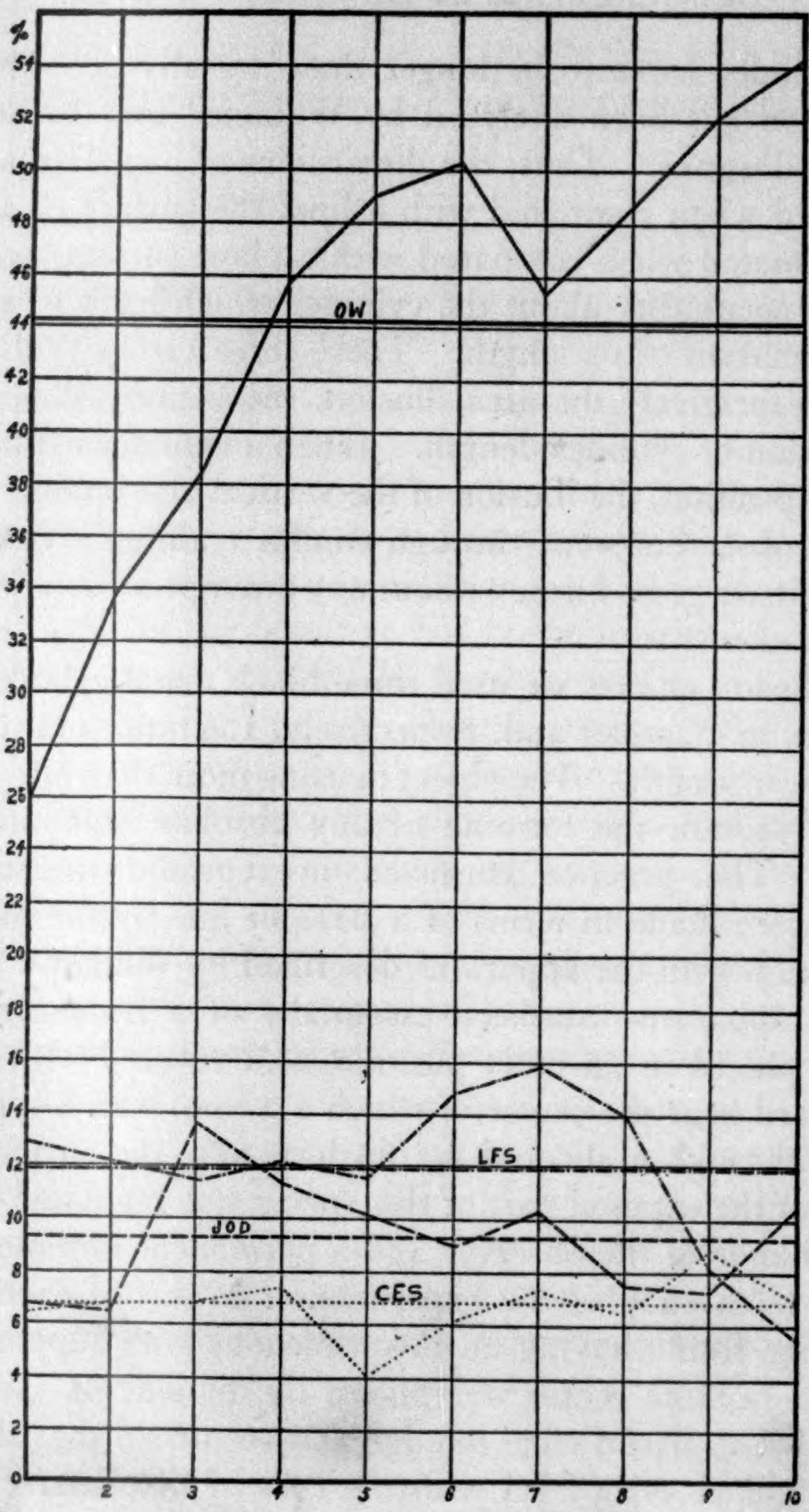


FIG. I. Horizontal Position.

Observations were made on both the vertical and the horizontal positions of the cylinders. The vertical length was measured in terms of a vertical line and the horizontal length in terms of a horizontal line. Repeating this on the three cylinders made six independent sets of observations. Ten settings were made for each of

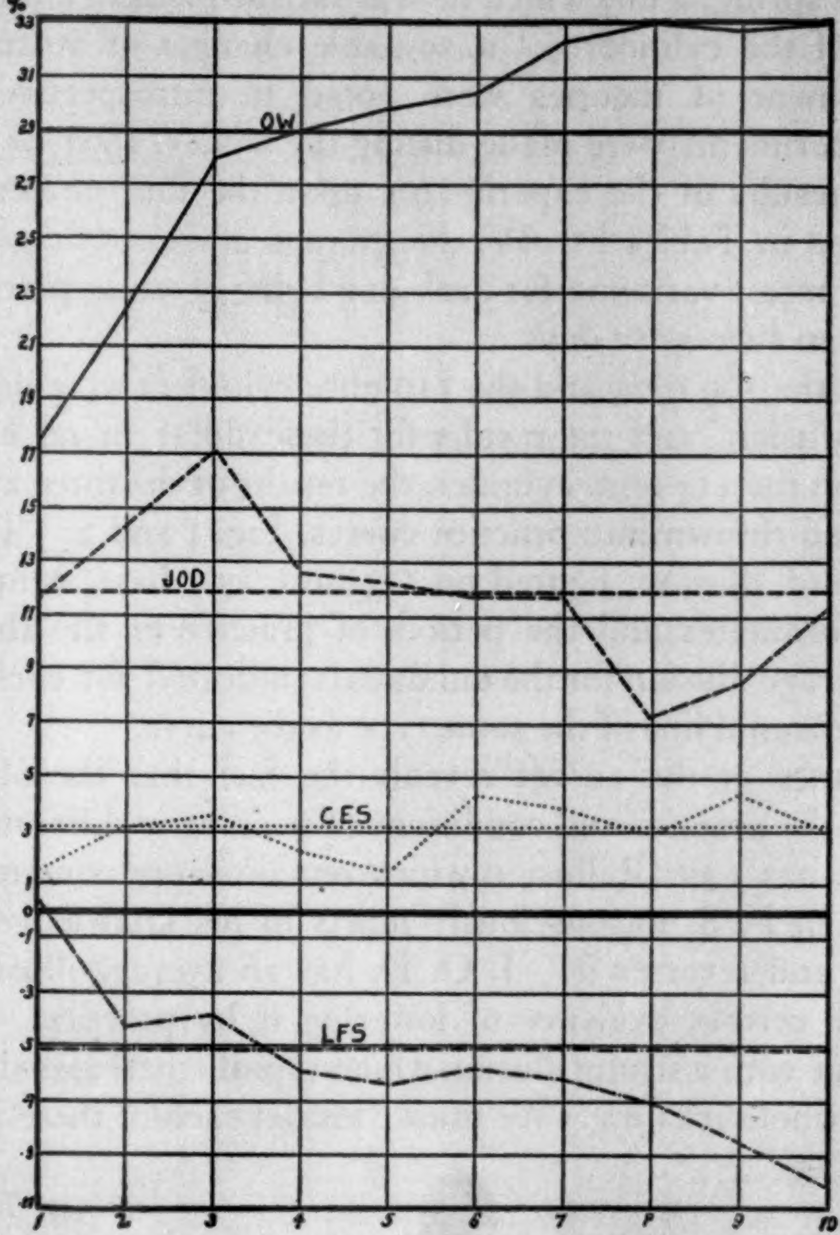


FIG. 2. Vertical Position.

these in each training period, which usually lasted an hour. The trials were made in the double fatigue order. The observers were allowed to look back and forth from the cylinder to the line as often as they wished, but were required to turn the head and not merely the eyes. All conditions of the experiments were

kept as nearly constant as possible. The experiments were made at the same time of the day and, as nearly as possible, on ten consecutive days. The observers were kept in complete ignorance about their results throughout the entire series. The task before the observer was quite simple—merely to represent, with the spring, a line which he was satisfied looked equal to the length of the cylinder. Unavoidable changes of attitude and development of theories were noted in introspective notes. The experiments were made during the winter, 1903–04.

The results of the experiments upon the four observers are contained in Tables I to IV, the average amount of the illusion and the mean variation for each day being given in percentages for the ten successive days.

Since the 109 mm. and the 119 mm. cylinders were used only for 'confusion' and the results for these differ in no essential way from the 114 mm. cylinder, the results of the three are combined and thrown into practice curves, Fig. 1 and 2. The percentage of illusion, figured on 114 mm. as a base, is indicated on the ordinates and the periods of practice on the abscissae. The average illusion for the ten days is indicated for each curve by a horizontal line of the same type as the curve.

A glance at the curves reveals the fact that the observers react to the practice and conditions of training in different ways. C. E. S. has a small illusion which remains approximately constant. L. F. S. unconsciously reacts to his knowledge of the illusion and reverses it. J. O. D. has an average illusion and gives no certain evidence of lowering it by practice. O. W. starts out with a strong illusion which rapidly increases throughout the whole training. We must consider each of these in some detail.

C. E. S. had full knowledge of the nature and force of the illusion, as well as of all the conditions of the experiment. He had had varied and extensive experience in the analysis and measurement of all the illusions involved. He expected the series to start with an illusion of about 10% for the vertical position, but did not have so definite expectation in regard to the horizontal position. Steadily avoiding to 'think it out,'

TABLE I. (C. E. S.).

A. Horizontal position.

	109		114		119		Ave.
Day	% Il.	% m.v.	% Il.	% m.v.	% Il.	% m.v.	% Il.
1	5.5	1.8	5.3	1.8	8.4	2.3	6.4
2	7.3	1.8	6.1	1.7	6.7	1.7	6.7
3	7.3	1.8	6.1	1.8	6.7	2.3	6.7
4	7.3	1.8	7.0	0.9	7.6	2.3	7.3
5	3.7	0.9	5.3	2.7	3.7	2.3	4.2
6	6.4	1.8	4.4	1.8	7.6	1.6	6.1
7	7.3	1.8	7.0	0.9	7.6	1.7	7.3
8	5.5	2.6	6.1	1.7	7.6	2.3	6.4
9	9.2	1.8	7.9	1.8	9.2	1.6	8.8
10	7.3	1.8	5.3	1.8	8.4	1.7	7.0
Ave.	6.7	1.8	6.1	1.7	7.2	2.0	6.7

B. Vertical position.

	109		114		119		Ave.
Day	% Il.	% m.v.	% Il.	% m.v.	% Il.	% m.v.	% Il.
1	2.7	1.8	1.8	1.8	0.0	2.3	1.5
2	3.7	2.7	3.5	1.7	2.3	1.6	3.2
3	2.8	1.8	4.4	2.6	3.4	2.3	3.5
4	1.8	1.8	2.6	1.8	2.3	1.7	2.1
5	1.8	1.8	2.6	1.7	0.0	1.6	1.5
6	3.7	1.8	5.3	2.6	4.1	1.7	4.4
7	4.6	1.8	3.5	1.8	3.4	1.6	3.8
8	3.7	1.7	2.6	2.6	2.3	1.6	2.9
9	3.7	2.7	5.3	1.7	4.1	0.8	4.4
10	3.7	2.7	2.6	1.8	3.4	1.6	3.2
Ave.	3.2	2.1	3.4	2.0	2.5	1.7	3.0

TABLE II. (L. F. S.).

A. Horizontal position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	13.8	2.7	12.3	5.3	12.6	2.3	12.9
2	12.8	2.7	14.0	1.7	10.0	1.6	12.3
3	13.8	0.9	11.4	3.5	9.2	1.6	11.1
4	14.7	2.7	12.3	2.6	9.2	2.3	12.2
5	12.8	1.8	11.4	1.7	10.9	2.3	11.7
6	15.6	1.8	14.9	1.7	13.4	1.6	14.6
7	17.4	1.8	15.8	1.7	14.3	1.6	15.8
8	12.8	1.8	13.2	2.6	16.0	0.8	14.0
9	9.2	1.8	7.0	0.9	7.6	1.6	7.9
10	5.5	2.6	6.1	0.8	5.0	1.6	5.5
Ave.	12.8	2.1	11.8	2.2	10.8	1.7	11.8

B. Vertical position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	3.7	0.9	− 0.9	1.7	− 1.6	1.6	0.4
2	− 3.7	1.8	− 2.6	1.7	− 6.7	0.8	− 4.3
3	− 2.7	1.8	− 3.5	1.7	− 5.0	2.3	− 3.7
4	− 3.7	3.7	− 6.1	1.8	− 7.6	1.6	− 5.8
5	− 3.7	1.8	− 7.9	1.7	− 7.6	3.4	− 7.1
6	− 4.6	2.7	− 6.1	1.8	− 6.7	1.6	− 5.8
7	− 4.6	1.8	− 6.1	1.8	− 7.6	2.3	− 6.1
8	− 5.5	1.8	− 7.0	1.7	− 8.4	1.6	− 7.0
9	− 9.2	1.8	− 8.8	1.8	− 9.2	1.7	− 9.1
10	−11.0	1.8	− 8.8	1.7	−10.9	1.6	−10.2
Ave.	− 4.5	2.0	− 5.8	1.7	− 7.1	1.8	− 5.8

TABLE III. (J. O. D.).

A. Horizontal position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	7.8	0.9	6.1	2.6	6.7	1.6	6.7
2	7.3	2.7	6.1	3.5	5.9	2.3	6.4
3	12.8	2.7	14.9	2.6	13.4	2.3	13.7
4	12.8	2.7	10.5	1.7	10.9	1.6	11.4
5	9.2	1.8	12.3	1.7	9.2	2.3	10.2
6	6.4	2.7	10.5	1.7	10.0	2.3	9.0
7	11.0	2.7	11.4	2.6	8.4	1.6	10.2
8	7.3	2.7	7.9	1.7	7.6	2.3	7.6
9	5.5	1.8	7.9	2.6	8.4	2.3	7.3
10	6.4	1.8	11.4	2.6	13.5	1.6	10.4
Ave.	8.6		9.9		9.4		9.3

B. Vertical position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	11.9	1.8	12.3	1.7	10.9	1.6	11.7
2	16.5	1.8	14.9	2.6	12.6	2.3	14.7
3	18.3	1.8	17.5	1.7	16.0	1.6	17.3
4	13.8	1.8	12.3	2.6	11.8	2.3	12.6
5	11.0	1.8	13.2	0.9	12.6	3.4	12.0
6	11.0	1.8	12.3	1.7	11.8	2.3	11.7
7	11.9	1.8	12.3	1.7	10.9	1.6	11.7
8	7.3	3.7	6.1	4.4	8.4	3.4	7.1
9	7.3	2.7	9.6	3.5	8.4	2.3	8.4
10	11.9	1.8	11.9	1.8	10.9	2.3	11.6
Ave.	12.1	2.1	12.2	2.2	11.4	2.3	11.9

TABLE IV. (o.w.).

A. Horizontal position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	22.9	5.5	27.2	4.4	26.9	3.4	25.7
2	32.1	3.7	36.0	3.5	32.8	2.3	33.6
3	40.4	3.4	38.6	3.5	36.1	3.4	38.4
4	46.8	3.8	44.7	2.6	45.4	4.1	45.6
5	49.5	6.4	49.1	3.5	47.9	4.1	48.8
6	52.3	3.7	51.7	2.6	46.2	4.1	50.1
7	50.0	3.7	39.5	3.5	46.2	2.3	45.2
8	51.4	2.7	47.4	3.5	47.1	5.0	48.6
9	53.2	3.7	54.4	3.5	47.9	3.4	51.8
10	56.9	3.7	53.5	2.6	51.3	2.3	53.9
Ave.	45.6	4.0	44.2	3.3	42.8	4.0	44.2

B. Vertical position.

	109		*114*		*119*		*Ave.*
Day	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*	*% m.v.*	*% Il.*
1	17.4	3.7	19.2	3.4	16.0	3.4	17.5
2	27.5	3.7	21.9	2.6	18.5	3.4	22.6
3	26.6	3.7	24.6	4.4	32.8	5.0	28.0
4	29.4	5.6	28.9	7.0	28.6	6.8	29.0
5	33.9	4.6	24.6	5.3	30.2	2.3	29.6
6	32.1	3.7	32.4	4.4	26.9	2.8	30.5
7	35.8	2.8	30.5	3.6	31.1	2.3	32.5
8	35.8	3.7	33.3	2.6	30.3	3.4	33.1
9	32.1	3.7	36.0	3.5	30.3	2.3	32.8
10	33.9	4.6	34.3	4.4	31.1	3.4	33.1
Ave.	30.5	4.0	28.6	4.1	27.6	3.7	28.9

he continued throughout to feel that the illusion for the horizontal position would be less than for the vertical.

The average illusion in the horizontal position amounts to 6.7%; and in the vertical to 3%. There is no evidence of any constant tendency to change the force of the illusion with practice, and there is no appreciable progressive change in the mean variation. Both the per cent of illusion and the per cent of mean variation come out approximately equal for the three cylinders.

Among the introspective observations of C. E. S. the following are noteworthy:

"There is a much greater temptation to correct for the illusion of the vertical in the line than in the cylinder. It is more difficult to disregard a relatively simple illusion which readily intrudes in consciousness than one which is more complex and therefore becomes focal in consciousness only with effort. This may account for the difference in the force of the illusion in the vertical and the horizontal positions.

It takes greater effort to see the length of a cylinder than the length of a line. Looking beyond the near side of the cylinder gives distinct sensations of effort through the eye muscles. This peculiarity of effort is particularly noticeable in a visual comparison of the height of a cube or a square with the height of the cylinder. This suggests an explanation of the illusion of cylinder length which Williams isolated and determined quantitatively but was unable to explain on the data in hand at that time.[1]

There seem to be amœboid movements in the line; as one tries to size it up, there seems to be a creeping lengthening and shortening. This is traceable to change in the mode of regard; when one tries to get the line as a whole, there is a tendency to converge the eyes for a point beyond the plane of the line and, consequently, the line seems shorter—the retinal image being of a given size but the line being judged to lie farther away than it really is. This suggests an explanation for the illusion of filled space in a line which is simply bisected: the bisection favors the effort to regard the line as a whole and, therefore, the tendency to converge for a point behind the plane of the line.

It was soon noticed that these creeping, amœboid movements were invariably referred to one end of the line, the movable one. (Bear in mind that the line was represented by a spring which protruded through the background and that its length was changed by shoving the spring in or out.) The method employed afforded especially favorable conditions for the apperception of this effect. Ordinarily we are so sure that a line does not change in length while we look at it that we inhibit the actual sensory process. Here the line was being adjusted so frequently that I became particularly appreciative of movements, both real and apparent.

[1] Williams, op. cit.

There was a tendency to remember the length of the horizontal line and to be influenced by this absolute standard in the perception of the vertical. (For this observer, who was aware of the differences of the cylinders, this tendency may have contributed toward the difference in the illusion for the figures in the horizontal and the vertical positions.)

There is a tendency to turn the eyes instead of turning the head. This may account for the error often found in comparing the length of two objects some considerable distance apart. The one to which the eyes are turned without turning the head tends to be overestimated."

In order to determine whether the illusion is likely to be less when one feels particularly satisfied with a measurement, C. E. S. followed the plan of recording cases in which he felt especially sure and satisfied that the adjustment of the line was right. There are in all sixty-three such cases distributed as follows: Twenty-seven give a smaller illusion than the average for the day by an average of 2.0%; twenty-five give a larger illusion than the average for the day by 1.6%; eleven cases coincide with the average for the day. Taking the three groups together, they give an average mean variation of 1.5%. The average mean variation for the series is 1.9%; the 'sure' cases are, therefore, somewhat more reliable; and the number of these cases above the average illusion is about equal to the number below but those below are 0.4% farther away from the average than those above. Hence there is no prominent tendency except in the direction of a smaller mean variation.

L. F. S. was a graduate student with a keen and brilliant mind. He was familiar with the illusion of the vertical. He also knew that the cylinder looked longer than it really was, and this knowledge, though undifferentiated, was of a sort of bogey order because he had seen the illusion demonstrated in some extreme forms. He was not informed about the dimensions of the cylinders or the order in which they were presented.

His average illusion in the horizontal position is 11.8%. While there is a complete wave in the latter part of the curve, it cannot be said that there is any constant progressive change with the practice. His recognition of the area and the volume illusions on the ninth day, however, may account for the lowering of the curves on those days.

The average illusion for the vertical position is negative, aver-

aging – 5.8%, and in this there is a uniform increase throughout the practice. He starts with no illusion and ends with a minus record of – 10.2%. The mean variations remain approximately constant for both positions throughout the ten days.

In the introspective account written after the tenth day, but before knowing the results, the following items are particularly relevant:

"Feel that I have not improved during the course of the observations. If there is any change, so far as I know, it must be due to practice."

"Knew of the illusion of the vertical though I did not attempt to use this knowledge. The same is true of what I thought to be a 14% illusion of cylinder (length)."

"Of the three cylinders, I think that the longest two are equal in length and that, in one of these, the length is equal to the diameter and, in the other, the length is greater than the diameter. The length and the diameter of the short cylinder are also equal."

"I think that the illusion would be more pronounced in the vertical cylinder than in the horizontal. My records for the horizontal cylinders are more accurate than for the vertical. Occasionally I was aware of a tendency to make my judgment of the length of the vertical line by comparing it with what I remembered to be the length of the horizontal line for the corresponding cylinder. If then I would make the vertical seem equal to the horizontal, I would make it too short, on account of the illusion of the vertical."

"Given a cylinder in the vertical position and a line in the vertical position, I think there would be a tendency to make the line too short. I became more aware of this tendency during the latter part of my observations."

On the ninth day he records that, instead of regarding the whole cylinders, he imagined a line drawn from end to end on the near surface of the cylinder. This is important because it is a condition which would tend to eliminate the illusions in the cylinder. (See notes by C. E. S. above.) The same day he records that he had not before thought distinctly of the effect of the area and the volume illusions. These two changes in attitude easily account for the downward turn in the curve in Fig. 1 on the ninth and the tenth days.

The explanation, then, of the results for the training for L. F. S. are essentially these. For the horizontal position the illusion is normal to a person 'with knowledge;' the lessening of the illusion on the last two days is accounted for by the two changes in attitudes, named above. This lowering is, however, not

greater than the immediately preceding rise, for which we have no explanation.

The reaction in the vertical position is partly accounted for by (1) the tendency to correct for the illusion of the vertical in simple and not in complex forms, as noted in the case of the first observer; (2) the bogey character of the gross illusion of length in the cylinder in the vertical position. (This observer did not know the force of the illusion for the horizontal position, and his estimate of 14% for the vertical position was a conservative estimate with reference to his own critical and discriminative attitude which excluded such force of the illusion as may be due to indiscriminate estimates); (3) the fact that he thought that the illusion would be more pronounced in the vertical than in the horizontal cylinder; and, (4) the fact that he considered the record for the horizontal cylinder most accurate, there being a feeling of unrest with reference to the vertical. The extreme negative results for the last two days have been accounted for above.

J. O. D., a liberal arts junior, was a distinguished athlete, slow in all his movements but a keen observer. He had heard an elementary lecture on the type of illusions involved and knew that the experiment involved these, but he did not have specific data clear in mind as a basis for correction although he was able to name some of the illusions. He had had no practice on this or any other illusion. The general effect of his information was to put him particularly on guard against all possible sources of inaccuracy. He accepted, without much questioning, the plain directions to make every possible effort to improve in accuracy.

The average illusion for the horizontal position is 9.3%; and, for the vertical, 11.9%. In neither case is there any constant tendency toward progressive change with the practice.

On the third day the observer recorded that he had been making the lines too long this day and attributed it to excessive fatigue. This explanation can, however, not be accepted without taking into consideration that he reported the same kind of fatigue and dullness on the fifth and the ninth days, and the illusion was almost average on the fifth day and below average

on the ninth. On the fourth day he reported a tendency to allow for the volume illusion 'by making the line a little longer.' This allowance was, of course, in the wrong direction; its effect is seen especially in the curve for the horizontal position.

He also noted the change corresponding to the fall in the curve on the eighth day and rightly attributed it to a scheme of imagining a plain line on the face of the cylinder, just as L. F. S. had done on the ninth day.

This observer noticed the "amoeboid" movements of the line on the first day.

O. W., a bright and painstaking liberal arts freshman, was a naïve and unprejudiced observer. He had not studied psychology and knew practically nothing about illusions. Special care was taken to keep him from getting any suspicion of the existence of illusions during the experiment. As a good student, he took the instructions in good faith and worked most diligently in the daily effort to cultivate accuracy in the use of his eyes.

His average illusion for the horizontal position is 44.2%; and, for the vertical, 28.9%. In both there is an unmistakable progressive increase in the illusion.

The observer being untrained, and the experiment being conducted with the effort to maintain a naïve state of mind, no introspective account was obtained. When he was shown the results at the conclusion of the series he was shocked, simply surrendered in a sort of despair and had no explanation to offer.

Our experiments resulted in no simple law of the effect of practice, but they enrich our insight into the actual complexity of the process. The four observers each represent an individual type of practice effect.

In the eight curves there are three cases of marked progressive change—two of increase in the illusion, and one of increase in the over-correction for it. The other five curves indicate no progressive change. To one who knows the observers and the conditions under which they worked these results seem 'strangely' natural. The first three observers had knowledge of the illusion and this probably reduced its force by as much as one-half. C. E. S. had gone through so much general training in illusions as to be free from disturbing motives which are due to

lack of a true point of view or lack of a discriminative attitude. J. O. D. was objective minded and naturally maintained the sensory-discriminative attitude in which the more rigid motives for the illusion gained uniform expression. L. F. S., who had not had sufficient training to guard him against the danger of being influenced by a partial knowledge of the situation, gave way to his analytical tendencies and made semi-conscious corrections, progressively increasing, in the vertical position. O. W. started with the strong illusion characteristic for hose who have no knowledge of it and, finding the task increasingly perplexing, probably changed mode of regard, etc., but for the worse and the illusion gradually increased.

If the drop in the curve for L. F. S. had taken place in the presence of the normal illusion there would have been danger of interpreting it as a clear case of gain through practice, but here it is clearly shown to be merely an expression of prejudice. This case is therefore particularly noteworthy.

The rise of the curve for O. W. is also noteworthy because it takes place for a person who already has a very strong illusion.

There is no distinct progressive tendency to increase or decrease the force of the illusion during the individual sittings, except in the three curves which show the progressive change for the whole series. There are many temporary fluctuations in the curves which may be accounted for by changes in method, etc., which one cannot fully preclude, but we have here discussed these experiments only from the point of view of progressive change.

II. THE T-ILLUSION.

Measurements by Eva Crane Farnum.

If two straight lines are joined in the shape of a plain capital T the one which is bisected seems to be shorter than the other. This is true though in different degrees, in all positions of the figure, when the illusion of the vertical has been eliminated.

This illusion was selected as representing a type which is probably due to lack of discriminative observation. It is usually very strong for one who is not aware of it. It was thought that mere practice, without information, would lead quickly to

discriminative apperception which would eradicate the illusion.

The illusion has never been fully explained. An analysis of the figure reveals several motives. First, as the two lines are at right angles, the illusion of the vertical enters. When the bisecting line is in the vertical position, the illusion of the vertical coöperates with the T-illusion; but, when the bisecting line is in the horizontal position, the illusion of the vertical counteracts the T-illusion. This motive may be fairly eliminated by studying the figure in both vertical and horizontal positions.

The T-illusion proper may be reduced to several component factors, and it is not unlikely that different motives operate in different methods of judging.

(1) The single division of a straight line is one constant factor. It is well known that, while filled space is usually overestimated, there is a paradoxical exception[1] in the fact that a single interruption, such as a bisection, leads to underestimation. The cause of this is a small but rigid motive about which there are several well-known theories. (2) Contrast enters in that a short line is compared with a long line when, as is often the case, half of the whole line is compared with one end of the bisected line. This has been demonstrated in three forms, namely: (a) the double square, (b) the two sides of the double square in the shape of L, and (c) two plain horizontal lines, one twice as long as the other.[2] (3) Confusion of whole and half of the bisected line, impossible though it may seem, is probably the main motive for the illusion when it appears very strong, as in children or adults who lack power of keen discrimination. There is a sort of subconscious tendency to select a variable line that is shorter than the whole bisected line because there is a vague craving for comparison with the one-half of it.

Three observers engaged in a practice series taking a minimum of a half hour a day for twenty days in the most intensive form of practice under the given conditions. The days were

[1] Wundt, "Geometrische optische Täuschungen," 82.

[2] Seashore and Williams, "An Illusion of Length," Psychological Review, VII, 592.

nearly consecutive. Two of the observers were selected with the hope of obtaining naïve results and training without theory or knowledge of the records during the practice series. The third man was a psychologist familiar with the illusion.

The measurements were made by the method of selection. The T-figure was drawn on a series of fifteen white cards, 36 cm. square, in black ink with lines five-eighths of a millimeter in width. The bisected line was equal in all the figures, 114 mm. The other line was varied by five-millimeter steps from 79 mm. to 149 mm. The cards were so frequently changed as to prevent identification. These experiments were made in the fall of 1905.

The cards were exposed one at a time against a neutral background at a distance of one meter from the eye and at right angles to the line of regard. The observer was required to state at each exposure whether the undivided line seemed longer, equal to, or shorter than the bisected line. The experimenter followed a definite plan in presenting the cards, as follows: The experiment might begin with any card, but after that, the observer's reply determined which card should be presented next and the plan was so arranged that the cards were selected in a continuous zigzag crossing the region of equality. The procedure may be represented in the following scheme, in which the numbers denote the length of the variable lines and the letters denote the three respective judgments Longer, Shorter and Equal:

79	84	89	94	99	104	109	114	119	124	129	134	139	144	146
			S	S	E	E	L	L		106.5				
		S	S	E	E	E	L			104.0				

This method is very effective and enables one to work economically and without fear of bias or knowledge of the results.

Four series were made by placing the figures successively in each of the four cardinal directions. In speaking of the directions, we shall refer to the direction of the variable line as well as to the number of the positions, thus: (1) ⊥ vertical-up; (2) ⊣ horizontal-left; (3) T vertical-down; and (4) ⊢ horizontal-right.)

The practice was distributed equally among the four series and, on the average, about 130 judgments were made in each sitting.

Among the methods, as learned from the introspections, the following three were frequently used and will be referred to by numbers, as follows: (1) The judgment was based, upon the general impression of the figure as a whole without any separation into parts. (2) The standard or bisected line was superposed upon the variable or undivided line. (3) One end of the standard, or bisected, line was rotated through 90°, using the point of bisection as a center of rotation.

The results are represented in Tables V, VI and VII and are represented graphically in the corresponding curves, Figs. 3, 4 and 5.

Since the four positions of the figure naturally divide themselves into two pairs, the vertical and the horizontal, the results for the four sets are grouped into two sets, namely, 1–3 and 2–4. For our present purpose such combination offers no objection. The results are expressed in terms of percentages, based upon the standard, 114 mm. Each record shows the average per cent of illusion for the day, with the per cent of mean variation.

The records are the gross results, without the elimination of the illusion of the vertical or any analysis. They simply mean that, under a given condition, the variable line was selected so much too short, or too long as the case may be. The minus sign denotes that the variable was selected longer than the standard.

D. H. was a freshman student in the university. She was a careful and painstaking observer of more than average intelligence. She had been a pupil of the experimenter in the preparatory school. This made her feel at home with the experimenter and favored a natural and docile attitude without any feeling of restraint. She did not know that an illusion was being measured nor did she have any specific knowledge about illusions. She regarded the experiment as an opportunity for accurate sense training and expected to acquire skill.

As the aim was to determine what mere persistent effort, without information, would accomplish, no introspections could

TABLE V. (D. H.)

Day	⊥ and T %Il.	⊥ and T %m.v.	⊣ and ⊢ %Il.	⊣ and ⊢ %m.v.
1	22.8	3.1	11.1	3.2
2	25.3	2.1	8.9	2.1
3	22.7	1.4	7.5	2.4
4	21.5	2.7	6.4	2.0
5	19.7	0.4	7.8	1.6
6	22.7	1.6	9.9	1.7
7	22.3	1.5	10.4	1.5
8	21.9	2.4	8.9	2.2
9	22.5	2.4	3.6	2.0
10	18.8	1.3	7.7	1.7
11	20.3	1.9	7.9	1.4
12	21.3	1.9	4.7	2.5
13	22.3	1.2	7.1	1.7
14	22.6	2.3	6.8	2.2
15	22.9	1.6	6.8	2.1
16	23.6	1.1	4.4	1.6
17	23.0	1.2	4.7	1.0
18	23.2	1.3	5.7	1.3
19	24.5	1.6	5.8	0.9
20	26.3	0.0	4.9	1.2
Ave.	22.5	1.6	7.0	1.8

TABLE VI. (T. S.)

Day	⊥ and T %Il.	⊥ and T %m.v.	⊣ and ⊢ %Il.	⊣ and ⊢ %m.v
1	22.8	3.1	0.1	3.8
2	21.2	1.8	− 2.6	1.8
3	18.8	1.6	− 4.8	1.8
4	19.2	1.8	− 5.7	2.2
5	14.6	1.3	− 7.1	1.4
6	15.4	1.3	− 6.6	1.6
7	14.3	1.7	− 4.8	1.6
8	13.7	1.8	− 6.0	1.8
9	13.2	1.5	− 5.1	1.5
10	11.5	1.1	− 7.7	1.5
11	9.5	0.7	− 9.3	1.5
12	9.9	1.5	− 7.1	1.1
13	9.9	1.3	−10.1	2.2
14	9.9	0.7	−12.2	1.1
15	10.2	1.3	−11.5	1.8
16	10.2	1.3	− 9.3	1.5
17	11.5	0.8	−11.3	1.6
18	11.7	1.3	−15.0	1.3
19	13.3	1.3	−14.1	1.7
20	16.8	1.9	−14.4	1.7
Ave.	13.9	1.5	− 8.2	1.7

TABLE VII. (C. E. S.)

Day	⊥ and T %Il.	⊥ and T %m.v.	⊣ and ⊢ %Il.	⊣ and ⊢ %m.v.
1	8.5	1.8	− 6.0	2.7
2	6.6	2.2	− 9.6	2.1
3	5.8	1.6	− 7.6	1.9
4	1.1	1.6	−11.5	1.2
5	1.1	1.7	−10.8	1.7
6	− 0.4	2.3	− 9.3	1.3
7	1.7	2.0	− 7.3	1.7
8	6.2	1.9	− 6.0	1.3
9	5.3	1.1	− 6.4	1.6
10	7.1	1.2	− 4.2	1.2
11	8.0	1.0	− 4.6	1.8
12	7.1	1.2	− 7.9	1.1
13	4.7	0.6	−11.0	1.4
14	4.4	0.7	−10.8	1.9
15	3.8	2.0	−11.3	1.2
16	1.3	1.1	−14.1	1.0
17	1.1	1.5	−12.1	1.3
18	0.7	1.0	−14.8	1.1
19	0.9	0.9	−15.0	0.9
20	1.3	1.3	−13.9	1.3
Ave.	3.8	1.4	− 9.7	1.5

be asked for until the series had been completed and but little was volunteered.

Reference to Table V and Fig. 3 shows that for the vertical positions of the variable her average gross illusion on the first

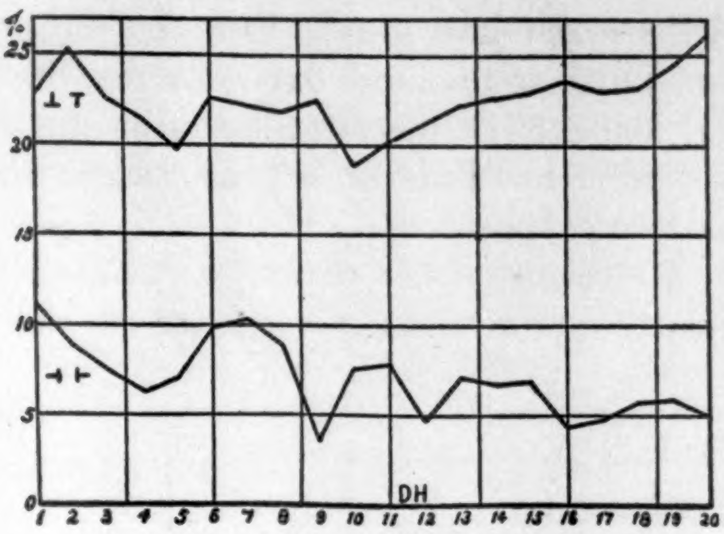


FIG. 3

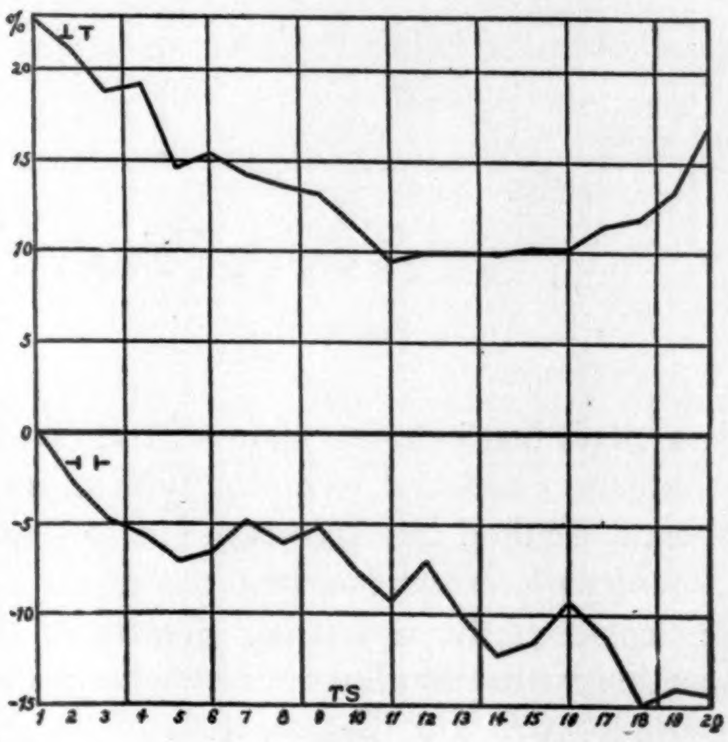


FIG. 4

day was 22.8 % and, on the last day, 26.3 %, there being a slight decline for the first ten days and a somewhat larger rise during

the second ten days. The average illusion is 22.5%, the minimum being 18.8% (tenth day) and the maximum 26.3% (last day). The table shows that the mean variation is small and regular.

For the horizontal position, the general average for the twenty days is 7%, the maximum, 11.0%, being on the first day and the minimum, 3.6%, on the ninth day. On the whole there is a slight general tendency in the direction of decline of the illusion. This decline but little more than offsets the increase shown for the vertical position.

To obtain a measure of her normal capacity in visual perception of space when no illusion was involved, she was tested

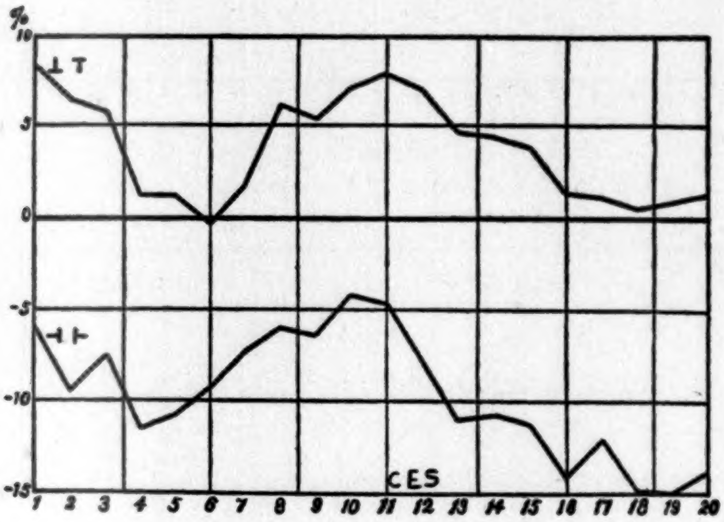


FIG. 5

on making one plain line equal to another, the two lines being in the same direction and end to end. With a line 114 mm. long, thirty trials resulted in an average error of 0.6% with a mean variation of 1.4%, which is a good record.

At the conclusion of the series, her illusion of the vertical was also measured with plain lines. Here the method of production was employed. The illusion of the vertical, when the variable line was in the horizontal position, averaged 16%; but, when the variable line was in the vertical position, she showed a strong tendency to correct for the illusion of the vertical. This tendency has been demonstrated before and is

due to the fact that the effort to adjust the vertical line makes the conditions for the illusion focal in consciousness, which is not the case in the other position. We may therefore take the record for the horizontal position as the truer record of her illusion of the vertical. This procedure is the more justified in view of the fact that the measurement was made after the training had been completed.

The attempt to eliminate the illusion of the vertical in cases like this is fraught with many dangers of error; there are a number of known, and doubtless also unknown, factors which should enter into our calculations, but we may make a gross elimination and bear in mind that the result is subject to minor corrections. Making the illusion, in round numbers, 15%, we find the data for the two positions self-consistent; for 22% minus 15% leaves a residual of 7% as the measure of the T-illusion in the vertical position; and, 15% minus 7% leaves a residual of 8%, which is the average record for the horizontal position.

We must guard against being misled by the difference in the level of curves in the charts. There is danger of assuming from a glance at the chart that the T-illusion is greater in the vertical position than in the horizontal.

For this observer, then, the T-illusion amounts to about 7%; in one position of the figure, there is a small increase and in the other, an equal decrease in the gross illusion. Therefore we may say roughly that the illusion is comparatively small and is uninfluenced by training without knowledge.

Observer D. H., according to remarks during the experiment and careful examination afterward, maintained a naïve attitude throughout the whole training. She followed her natural intuitive tendencies and reported her impression of the figure at each exposure without knowledge of danger or serious suspicion of sources of error. She trusted her eyes. Indeed she used different methods and commented on the reasons for change but spoke of them merely as "difficulties," giving the impression that they spurred her on to use her eyes carefully. She thought that she was learning how to see the figure most effectively, her confidence in her judgments increased and she felt that she was improving with the practice.

The data in hand would seem to justify the following interpretation of her case: She had a large illusion of the vertical which probably was not allowed for, or affected by the practice; she had a small T-illusion, probably due to the small and constant motives, which also remained practically constant throughout the practice. The practice resulted in a slight decrease in her mean variations for the daily records.

T. S. was a freshman, not exceptionally bright, but a faithful student who always secured good records in his class work. He knew nothing of psychology and approached the subject in the same naïve attitude as the foregoing observer, except that he was fully aware of the illusion of the vertical. He was asked not to make any allowance for this but simply to trust his eyes regardless of any theories he might have.

He started with a gross illusion of 22.8% which rapidly decreased to 9.5%, on the eleventh day, and then gradually increased to 16.8%, on the last day, making an average of 13.9%. In the horizontal position, his record starts with 0.1% and falls gradually to −14.4% on the last day, the average being −8.2%.

Tests for accuracy in space perception when no illusion is involved, as, e. g., in the above test on Observer D. H., show that in this respect he has greater ability than the average male university student. He made an average error of .2%.

Tests for the illusion of the vertical with plain lines, at the end of the series, revealed a strong illusion of the vertical—about 12%. As this record was obtained after the training series, it is safe to assume that the illusion of the vertical had not decreased much in the training. If we assume the conservative estimate of 10% as a constant illusion of the vertical, the T-illusion began with a force of 12.8% and fell off rapidly and steadily, during the first ten days, until it was eradicated; it remained absent for about six days and then gradually returned during the last four days.

Making the same allowance of 10% for the illusion of the vertical in the other position, we find that there is a very consistent parallel in the force of the T-illusion for the first sixteen days. During the last four days there is an end to this correspondence.

The general conclusion, then, would be that, this observer starts with a strong T-illusion which gradually passes away but returns with about half its original force in the vertical figure and is overcorrected at the same time in the horizontal figure.

Observer T. S. revealed by his remarks during the experiment and by examination after the experiment that he took an attitude of speculation and developed several theories which undoubtedly influenced him, although he tried to follow the instructions to make no allowance for any theory of sources of error. Thus, he learned by comparing different methods of judging the lines, that the bisection had the effect of shortening the line and he estimated this error to be possibly 20%. As a matter of fact, that motive does not amount to more than three or four per cent. This then, was a discovery of one of the true motives for the illusion, but it was overestimated and this overestimation led to the speedy decline in the record. Then, he developed a theory that the horizontal variable looks shorter than it really is, which is contrary to the T-illusion motive. He admitted a temptation to select a longer line on this theory, which would again tend to reduction of the illusion. This theory was, of course, based upon his knowledge of the illusion of the vertical and should be properly interpreted as resulting in a correction for the illusion of the vertical rather than an overcorrection for the T-illusion. This observer was aware that he got different results by different methods but the large changes in the curve are not due to any one method, because he changed methods frequently.

C. E. S. knew the details of the theory and the conditions of the experiment. He made an effort not to make any allowance for any of the known motives of illusion. From a former training series in the illusion of the vertical, he expected that to be about 6%. He expected the T-illusion to be considerably larger than that.

The gross illusion in the vertical position starts at 8.5% and falls rapidly, reaching zero on the sixth day; then it rises at about the same rate and reaches its original force on the eleventh day, and then falls again, reaching close to zero on the last five days.

The direction of the curve for the horizontal position is

fairly parallel to the curve for the vertical position. But the signs are minus, i. e., the horizontal line was made too long and the numbers in the low portions of the curve are too large to be expressions of the illusion of the vertical. This gives a clue to the interpretation of the large waves in both curves; namely, placing the normal illusion of the vertical constant at 6%, the drop below that number in either curve, regardless of sign, represents overcorrection for the T-illusion. This observer not only started with the T-illusion practically eliminated, but over-reacted against it unconsciously.

He represents the type of observer for whom the T-illusion is practically absent on account of training in accuracy of observation. But he started the training not knowing this, supposing that his records would show a decided T-illusion. It is well known that a conviction of that sort is almost sure to show itself subconsciously in some way. Here it resulted in periodic tendencies to overcorrect.

Although full notes are at hand, it is not easy to account in detail for these fluctuations. Of course, the observer did not know that there were any fluctuations until after the series was completed. From comparison of methods, chiefly the three mentioned, he concluded that the first (unanalyzed impression) would lead to the largest illusion, and the third (turning one half of the bisected line upon half of the other) was the most exact. In difficulties he used all three methods although he gave most weight to the third. But there is no traceable connection between the development of method and the notes on changes in method to correspond to the large waves in the curve.

After the nineteenth trial, he states:

"I cannot get myself to accept the third method 'straight' because it differs so much from the second. Still, at the present time, I approach the third method requirement more nearly than the second. I really believe that if I followed the third method rigidly there would be no error. I feel that there must be an illusion due to the bisection of the variable line which counteracts the T-illusion."

In the final introspection he says:

"I have continually struggled to remain in a naïve state of perception and to avoid making allowances. Thus, e. g., I have not figured out what the balance of all the illusions ought to be when the vertical line is in the horizontal position.

Still I have continually been conscious of a sort of allowance for the illusion of the vertical and the T-illusion, not in the sense of correcting fully for them, but in the sense of bearing in mind that there is a deceptive appearance. The judgment always impressed me as being very complicated and it has been difficult to keep the various factors constant. About the middle of the series I was particularly pleased with the 'Horizontal-left' figure. I felt that my judgment must be about right and thought that the two illusions probably cancelled. But the last few days I have grown more helpless and feel the greatest difficulty in this position."

An interesting index to this observer's orientation is obtained from a series of notes made from time to time saying which figures seemed to him to look exactly right and estimating what he thought might be right after corrections. On the fourth day he thought that the line which was −13% was probably the 'equal' figure in the vertical position. His record for that day was − 11.5%. On the fifth day he selected the −13% figure again as the 'equal,' *to his eye*, but estimated that, owing to the presence of the illusions, it was probably actually −4%. On the fourteenth day he was sure that the −9% figure was the true 'equal' figure, after due allowances for the illusions. We see in all three of these cases that the observer was under the impression that he had not corrected for the motives of the T-illusion, the error of his estimates being 13% in the first case, 9% in the second case, and 9% in the third.

To bring together the conclusions obtained from this inspection of the three records, we may say: One observer had a medium T-illusion which remained unaffected by practice; another observer started with a strong T-illusion which rapidly passed away, but returned in part for one position and resulted in a decided overcorrection in the other; the third observer had no T-illusion at the beginning but periodically over-reacted against the motive.

A general review of these facts would lead us to conclude (1) that the T-illusion may be due to either or both of two types of motives: first, failure to take a discriminative attitude toward the figure and, second, the presence of such more rigid motives as those which condition contrast and the underestimation of a bisected line. Observer D. H. was probably influenced only by the second type of motives, Observer T. S. by both, and

Observer C. E. S. only by the second. But C. E. S. labored under an exaggerated estimate of the force of these motives of the second type. (2) Where the motives of the second type are not known, the T-illusion is not likely to be affected by practice. (3) Where the motives of the first type are present at the beginning, they rapidly disappear with practice. (4) Suspicion of a large motive for illusion leads to unconscious correction.

It is fairly certain then, also, that wherever the T-illusion appears in very great force on first trial, it will decline with practice whether the observer proceeds with or without knowledge in the practice. Or, perhaps more to the point, the molives of the first type disappear the moment the observer takes that serious discriminative attitude which he naturally assumes in entering upon a training series.

The difference between the results for the members of each pair of figures may be noted in passing. In the horizontal position the 'left' line is made approximately one per cent shorter than the 'right.' This is in accord with the known tendency for equal distances to appear greater when at the left than at the right. In the vertical position the difference is greater. The 'up' line is made fully three per cent shorter than the 'down.' This is also in accord with the known tendency to overestimate the lower portions of a figure. In both positions the point of union of the two lines is naturally taken as the center of gravity of the figure.

III THE MUELLER-LYER ILLUSION.

Measurements by Eva Crane Farnum.

The conventional double fledged Mueller-Lyer figure was used in this study of practice-effect. Two series of training were run parallel, one by the method of production and the other by the method of selection.

In the method of production, Judd's Mueller-Lyer apparatus as described in The Yale Studies, N. S., I, 68–9, was used. This apparatus enables the observer to vary one of the base

lines by both coarse and fine settings without otherwise disturbing the figure. The records are made automatically.

In the method of selection, the complete figure was drawn on a series of large cards, the base line in one section being varied by three-millimeter-steps in the series of cards. The mode of procedure was identical with that described for the T-illusion above. A fresh start was made after every two complete determinations, the object being to prevent the observer from inferring what relation the present judgment would hold to a foregoing one.

The constant base line was 100 mm. long, the angle lines 30 mm., and the angles of these with the base line 45° or its complement. The section with converging angle lines was kept constant and always at the left by both methods.

The two methods were used together in order to make the practice as free as possible from the contingencies of method. The production method is rapid but is always subject to dangers of accessory parts in the figure and disturbing tendencies in movement. The only real dangers in the method of selection lie in the possibility of suggestion by the order of the presentation of cards and the possibility of identifying cards. The latter danger was guarded against by continually changing cards, and the former was eliminated by the order of procedure mentioned above. It is absolutely essential, in a test of this sort, that the observer shall have no means of determining objectively or by inference what the actual proportions are.

A day's practice consisted in twenty settings by the method of production and twenty complete determinations by the special method of selection. As each complete determination by this latter method involved from four to seven separate judgments, it afforded the largest practice, about one hundred and thirty judgments a day. After the first day or two, all this could be accomplished in thirty or forty minutes, if there was no interruption or rest.

Four observers engaged in the training; two for twenty-four, one for twelve, and one for thirty-five days. These experiments were made in 1906.

The results are condensed into Tables VIII, IX, X, and XI,

which give the record for each method, with mean variation, for the successive days. The method of production is designated by *P* and the method of selection by *S*. The figures are averages for the day and are expressed in percentages.

Table VIII. (c. e. s.)

	S		*P*	
Date	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
Feb.				
23	9.1	1.1	12.1	1.1
24	10.0	1.0	10.5	1.4
26	10.7	1.5	8.7	1.5
27	10.0	0.6	8.9	1.3
28	8.7	0.9	7.1	2.0
Mar.				
1	7.3	1.1	5.6	1.2
2	5.7	0.3	5.1	1.0
3	4.5	0.9	3.2	1.2
5	3.2	1.1	2.9	1.8
6	3.1	0.7	2.6	1.4
7	3.5	0.7	4.1	1.5
8	3.4	0.8	3.4	1.2
9	2.0	0.9	3.2	1.1
10	1.4	1.0	0.5	1.2
12	1.4	1.2	− 0.7	1.2
13	1.2	0.7	− 1.4	1.4
14	0.4	0.5	− 1.1	1.0
15	1.2	1.1	− 1.1	1.1
16	− 1.4	1.0	− 2.9	1.6
17	0.6	0.9	− 3.8	1.2
19	0.1	0.4	− 2.1	1.1
20	0.1	0.5	− 4.5	1.2
21	0.5	0.8	− 3.6	1.4
22	0.4	1.2	− 3.5	1.7

Table IX. (d. s.)

	S		*P*	
Date	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
Feb.				
26	11.9	1.3	12.9	3.3
27	10.7	0.9	14.0	1.9
28	12.6	1.8	14.3	2.0
Mar.				
1	10.3	1.3	9.8	1.4
2	7.2	1.4	8.6	1.8
3	6.6	1.1	7.0	2.0
5	2.2	1.4	3.7	1.1
6	4.7	1.9	6.5	1.4
7	4.5	0.6	5.2	1.3
8	4.2	0.8	3.8	1.0
9	1.2	1.0	4.4	1.1
10	1.0	1.7	5.2	1.7
12	0.0	0.5	4.1	1.2
13	− 0.1	0.4	7.3	1.0
14	0.4	0.5	6.7	1.0
15	− 1.2	1.7	3.8	2.3
16	− 2.8	0.4	0.8	0.7
17	1.1	0.8	2.6	0.8
19	0.3	0.6	0.1	1.0
20	0.2	0.3	1.1	1.2
21	− 1.1	1.2	1.9	1.2
22	− 3.3	0.9	1.0	0.9
23	− 2.3	0.9	2.7	1.0
26	− 1.1	1.0	1.7	1.5
27	− 0.7	0.9	3.1	1.1
28	− 1.9	1.1	3.0	0.9
29	− 1.9	0.7	2.2	1.2
30	− 2.3	1.1	2.1	0.6
31	− 4.3	0.9	1.4	0.8
Apr.				
2	− 3.3	0.8	1.8	1.1
3	− 4.1	0.7	1.2	0.5
4	− 3.5	1.0	3.0	1.3
5	− 2.7	1.2	2.0	0.5
6	− 2.9	1.1	2.2	0.5
7	− 2.9	1.0	1.9	0.6

TABLE X. (T. P.)

	S		P	
Date	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
Mar.				
9	15.7	2.1	21.5	2.8
10	18.6	1.2	16.8	0.9
12	20.4	1.1	16.5	0.8
13	19.5	1.0	19.2	0.5
15	21.3	1.4	19.7	0.5
16	24.7	1.2	20.4	0.8
17	21.9	1.2	20.1	0.4
19	21.9	1.0	21.9	0.7
20	22.0	1.1	21.0	0.6
21	22.0	1.2	21.7	0.8
22	22.8	1.0	21.3	0.7
23	22.0	0.9	21.8	0.7
24	21.7	0.9	21.7	0.7
27	21.5	1.2	20.9	0.4
28	21.0	0.4	20.8	0.4
29	20.1	1.1	20.5	0.5
30	19.6	1.1	20.9	0.9
31	19.5	0.9	18.4	0.8
Apr.				
2	19.9	1.0	19.8	0.6
3	19.6	1.0	19.7	0.4
4	19.3	1.0	19.9	0.6
5	19.2	1.1	19.9	0.6
6	19.3	1.0	19.6	0.8
7	19.0	1.0	19.7	0.7

TABLE XI. (J. A. M.)

	S		P	
Date	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
Mar.				
28	25.5	1.3	26.7	1.5
29	22.4	1.2	26.6	2.8
30	14.5	2.3	26.0	2.2
Apr.				
2	11.7	1.4	24.2	1.7
3	10.4	1.4	19.0	1.5
5	6.2	1.9	19.0	1.7
7	5.7	1.7	16.8	1.7
9	3.2	0.2	17.2	1.6
10	2.1	0.2	15.1	3.0
11	2.9	0.2	16.1	1.8
12	2.9	0.1	11.9	2.9
13	3.4	0.4	11.9	2.1

A minus sign indicates that the illusion was reversed. The same results are represented graphically in the curves. Figs. 6, 7, 8, and 9.

C. E. S. who was familiar with all the details of the experiment, started with a fairly strong normal illusion and this was gradually eradicated by the practice. The illusion disappeared on the fifteenth day by the method of production, and on the nineteenth day by the method of selection. The records for the two methods run fairly parallel until the last one-third of the series, when a progressive deviation begins. The records for the method of selection remain on a level, near zero for the last third of the series of days; but the other record continues to fall and is increasingly negative from the fifteenth day on.

The result was astonishing to the observer when he saw it after the series had been completed. From many years of experience with this illusion, and from practice on other illusions, he had been led to the conviction that no improvement would take place without conscious change in the perceptual

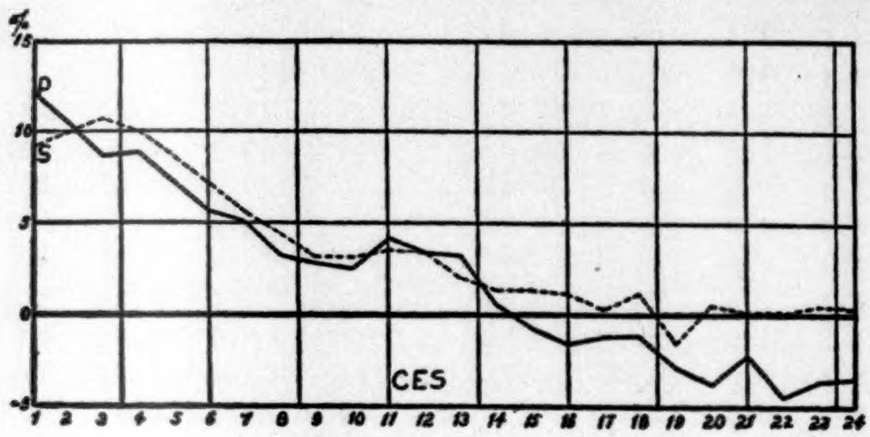


FIG. 6

attitude; and here he had remained in the same perceptual attitude toward the motives of the illusion, so far as it could be analyzed by introspection, from the second day to the end. He was under the impression that his normal illusion would amount to about 6% and that it would remain constant throughout.

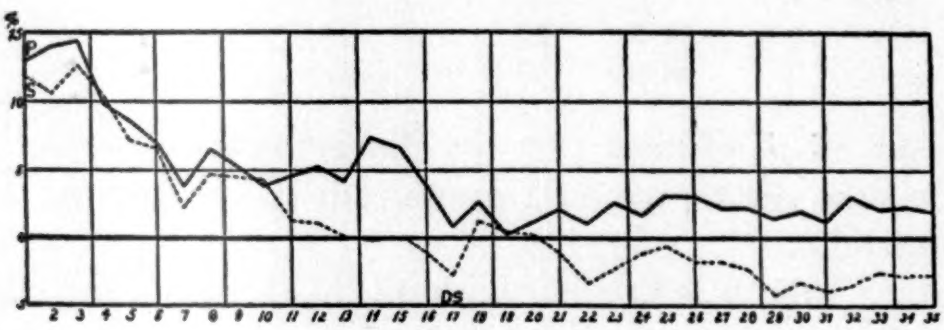


FIG. 7

From one point of view, this conviction was favorable to the reduction of the illusion for it kept the observer in that complacent attitude in which the perceptual process might adapt itself to the confronting difficulty without rousing consciousness of adaptation.

His method of judging was to fixate the middle joint and judge the two sections with the eyes at rest on this point, but

the act usually resulted in sweeping eye movements in both directions after the first fixation.

The difference between the *P* and the *S*-records in the latter part of the series was predicted by the observer during

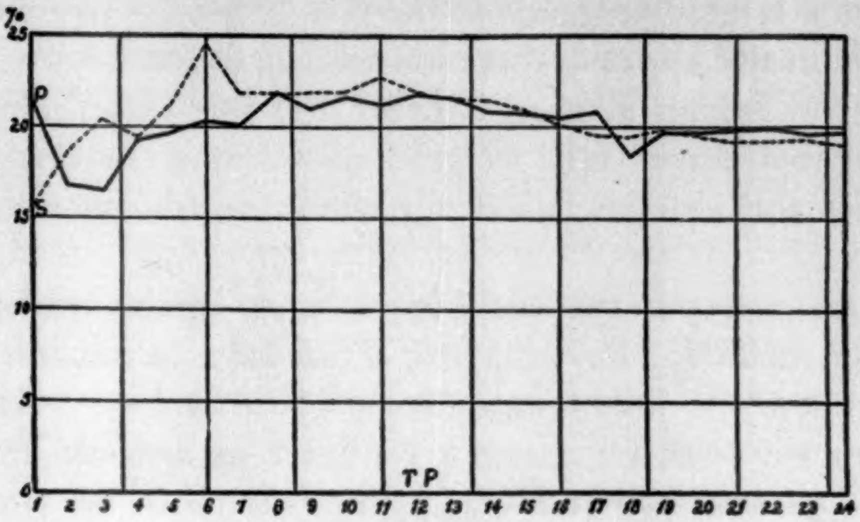


FIG. 8

those experiments. He recorded that the line of overlapping in the cards in the method of production was a source of confusion and he estimated that this might lead to the production

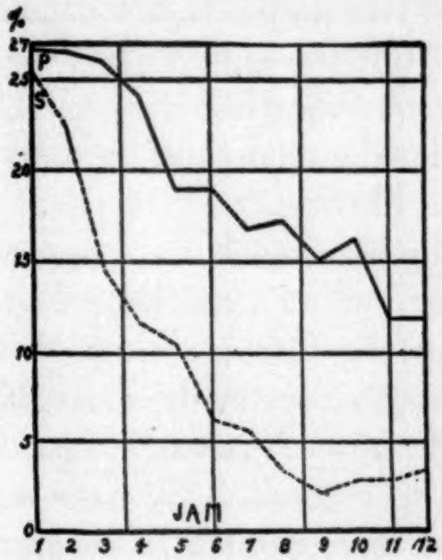


FIG. 9

of a longer line because a single interruption of a line leads to underestimation of that line. It is however difficult to see why the same principle should not have operated in the earlier part of the series when he was not aware of it as disturbing.

D. S. was also a trained observer thoroughly familiar with all the conditions of the experiment. So far as the effect of training is concerned, his records resemble those of the foregoing observer. The parallel is particularly close in the *S*-records. There is a large divergence in the results for the two methods, and it is in the opposite direction to that found for the former observer. Taking a mean between the two records, we may say that the illusion falls off gradually during the first half of the series and remains practically eliminated during the second half.

The main drop in this curve is explained by the introspective notes on method. For the first three days he recorded that he did not try to follow any particular method or contrivance but naturally took a glance at the figure as a whole and then sought a general impression as to the balance of the two parts to be compared. But on the fourth day he noticed that, "if I try to disregard the end-lines and center my attention upon the horizontal line, the left part has to be considerably longer than by the previous method in order to appear equal to the right." From the fourth day to the seventh, inclusive, he oscillated between these two methods but considered the latter the more satisfactory. On the eighth day, he fully adopted the second method and continued it to the end. During the transition period, he stated that the change of method must have resulted in a decrease in the illusion.

This record again illustrates the danger of constant errors, peculiar to the method of experimenting, entering into the record. Nothing in the introspections, in the critical review of conditions by the observer at the close of the experiment, or in the known conditions of the technique explains the divergence between the two records. We have no means of knowing which if either is the true one, further than the fact that the *P*-method is more complex and therefore more liable to error than the other.

The observer did not think that he had been influenced in his judgments by knowledge of the illusion. The force of the illusion is, then, accounted for in part by his method, or lack of method, for the first three days. The rapid decline following

is accounted for by the change of method. The further lowering is probably due to developed capacity for disregarding the accessory lines. The fact that the observer felt sure that the illusion had not been eliminated by the practice may have something to do with the overcorrection shown by the *S*-method.

T. P. was entirely naïve and uninformed so far as the psychology of illusions is concerned, and remained so throughout the experiment. He supposed that the accessory lines were there merely for the purpose of making the task of training more difficult, and never suspected the presence of any illusion. He was fully aware that he was to compare the base-lines only, but he was not aware that the accessory lines would influence his judgments. He therefore proceeded in the same way as a trained observer would in comparing two plain lines. It is easy to understand that it is no small achievement on the part of the experimenter to keep an observer so free from suggestions for twenty-four periods of experiment. This observer was a graduate student, with major in political science, extraordinarily faithful in his task, and confident of great good to result from his efforts.

He started with the strong illusion characteristic of those who are not aware of its existence and, although there are deflections in the curve, we may say in general that it remains at the level of about 20% without any tendency to change as the result of the practice. The mean variation is exceedingly small in comparison with the magnitude of the illusion, and the records by the two methods agree remarkably well.

A. M. was a graduate student in modern languages. His attitude may be expressed by quoting from notes made by him after the experiment had been completed. He says:

"I knew that 'things were not what they seemed,' before I commenced the experiments. But I had no idea as to what caused the illusion. I tried to estimate the length of the lines just as they appeared to me without making any allowance for any 'fake' I might imagine to exist. After a while the figures appeared different (?) without my making any allowance at all for the illusion. I did not attempt to figure out any system and did not think of the figures between experiments. The fact that I knew of the illusion may have unconsciously influenced my judgment, but I tried to guard against that."

The experiment was unfortunately interrupted, but it was carried far enough to reveal his type of reaction clearly. He started with the strong illusion of the uninformed but rapidly decreased this. The interpretation is undoubtedly to be found in the fact of a progressive adaptation of his mode of grasping the figure in a way favorable to isolation of the lines compared, but he made no effort to analyze the process. The difference between the results by the two methods is large.

Here again the different observers reveal different types of practice effect. In general the results may be summed up as follows: When the observer proceeds to the experiment with full knowledge of the conditions and does not expect the illusion to disappear, it does pass away, and without leaving any introspective evidence of the change; when the observer proceeds to the experiment without any knowledge of the illusion, and is not led to suspect any illusion, the force of the illusion remains unchanged throughout long continued practice; and, when the observer suspects the illusion but has no definite knowledge of its cause, it tends to disappear as in the cases of specific knowledge of it.

The difference between the results for the two methods of measurement employed shows the danger of ascribing to practice in general what may be due to peculiarity in method, and how practice may lead to improvement by one method of estimation and not by another.

Two years after the above training series had been completed C. E. S. and D. S. repeated the test to determine the effect of the long interval upon the practice gain. C. E. S., taking twelve complete determinations by the method of selection, revealed, an illusion of 12% with a mean variation of 1.4%. D. S., taking one hundred trials by the method of production, gave an average of 9%. The illusion had therefore returned to the approximately normal force that it had before the training. Unfortunately we have not yet had opportunity to repeat the training to determine to what extent the second training would profit by the first.

IV. THE ILLUSION OF DISTANCE BETWEEN CIRCLES.

Measurements by Raymond W. Sies.

The linear distance between two circles a moderate distance apart is overestimated. This is undoubtedly a form of the Mueller-Lyer illusion but it probably involves other motives than those ordinarily operating in the conventional form, as in the above experiments.

In the following experiments upon the effect of practice on this illusion the method of selection was employed as described in the section above, on the T-illusion. The standard figure consisted of two circles, each 114 mm. in diameter, the space between them being equal to the diameter of a circle. This figure was drawn on a series of cards, the inter-space being varied in successive cards by three-millimeter steps from 75 to 135 mm. The task was to select the card in which the distance between the two circles seemed to be equal to the diameter of a circle. Every effort was made to eliminate suggestion or information in regard to the illusion from the apparatus and method.

Three observers, representing as many different types of preparation, engaged in the test. Each observer made sixteen complete determinations each day for twenty successive days, Sundays excepted. This amounted to about one hundred judgments a day for each observer. The tests were equally distributed for the horizontal and the vertical positions.

Table XII shows the results for the three observers, giving the average illusion with its mean variation of the successive days for each of the three observers. Reversal of the illusion, i. e., underestimation of the distance between the circles is indicated by the minus sign. Fig. 10 represents these records graphically for the vertical position and Fig. 11 for the horizontal position.

C. E. S. is the trained observer who has taken part in the preceding experiments. His training on the illusion of the vertical, taken five years before, had resulted in no evidence of progressive gain from the practice. His training on the illusion of cylinder length had resulted in the same way. His train-

ing on the T-illusion had shown no appreciable progressive gain because the illusion was practically absent at the beginning of the training. The foregoing series of training on the Mueller-Lyer illusion was in progress and had reached the fourteenth day when the present series began, the observer of course being ignorant of what record he was making in the former until that

TABLE XII.

Day	Observer C. E. S.				Observer F. V.				Observer R. M.			
	Vertical		Horizontal		Vertical		Horizontal		Vertical		Horizontal	
	%Il.	*%m.v.*	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*	*%Il.*	*%m.v.*
1	7.2	5.0	0.8	2.8	2.6	3.7	1.5	1.0	17.8	2.7	5.9	3.1
2	4.1	1.9	−1.7	0.4	2.6	3.7	1.1	0.6	18.9	1.5	7.1	4.3
3	3.8	1.6	0.2	2.2	−0.2	0.3	−1.3	1.8	14.5	6.0	6.6	3.8
4	3.2	1.0	0.4	2.4	−1.1	0.7	−2.0	2.5	16.1	4.3	2.8	.0
5	2.6	0.4	−0.8	1.2	−3.9	3.5	−3.9	4.5	13.5	6.9	3.6	0.8
6	3.2	1.0	−1.5	0.5	−1.1	0.7	−0.4	0.9	14.6	5.8	−0.7	3.5
7	1.7	0.5	−2.8	0.8	−2.1	1.7	−0.4	0.9	14.6	5.8	−1.8	4.6
8	2.6	0.4	−2.3	0.3	0.0	0.4	0.5	0.0	16.6	3.9	−1.7	4.5
9	3.3	1.1	−1.8	0.2	0.4	0.8	0.5	0.0	17.1	3.3	0.0	2.8
10	1.8	0.4	−1.5	0.5	−2.6	2.2	−0.8	1.3	21.2	0.8	0.8	2.0
11	1.8	0.4	−3.0	1.0	−0.5	0.1	1.5	1.0	23.9	3.4	2.3	0.5
12	1.8	0.4	−3.3	1.3	1.0	1.4	0.2	0.4	22.2	1.8	2.3	0.5
13	1.7	0.5	−3.9	1.9	−1.7	1.2	−0.8	1.3	21.1	0.6	1.1	1.7
14	3.0	0.8	−1.1	0.9	−1.0	0.5	−0.2	0.7	21.7	1.3	2.0	0.8
15	2.0	0.2	−2.8	0.8	−1.0	0.5	1.3	0.8	24.8	4.4	2.3	0.5
16	0.2	2.0	−3.0	1.0	0.5	1.0	3.0	2.5	25.7	5.2	3.9	1.1
17	1.1	1.1	−2.5	0.4	0.8	1.2	3.6	3.1	24.8	4.4	4.7	1.9
18	0.4	1.8	−3.8	1.8	0.2	0.6	1.8	1.3	25.4	4.9	4.1	1.3
19	−1.8	4.0	−2.6	0.6	−0.5	0.1	3.3	2.8	27.0	6.5	4.3	1.5
20	−0.5	2.7	−2.6	0.6	−0.5	0.1	2.3	1.8	27.6	7.2	5.9	3.1
Ave.	2.2	1.4	−2.0	1.1	−0.4	1.1	0.5	1.4	20.4	4.0	2.8	2.1

series was completed, which was on the twelfth day of the present series.

The outcome of the training in the first three types of illusion had led us to assume that the overlapping of these two series would not interfere. The records show that this assumption was wrong and unfortunate. C. E. S. supposed that the illu-

sion at the beginning of this series would be about 8% for the vertical position and about 4% for the horizontal position. This estimate was based upon knowledge of the average illusion for students in the laboratory as well as upon measurements

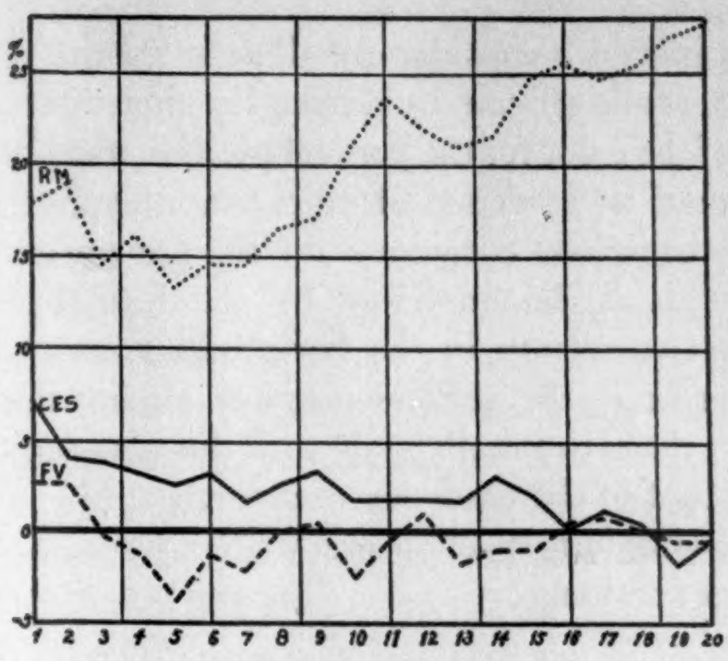


FIG. 10. Vertical.

made upon himself about seven years before. The results were therefore very surprising to him. They were doubly surprising, because after learning on the twelfth day of this series that the foregoing form the Mueller-Lyer illusion had tapered off with practice he had from that time supposed that the same process was going on for the present form of the illusion.

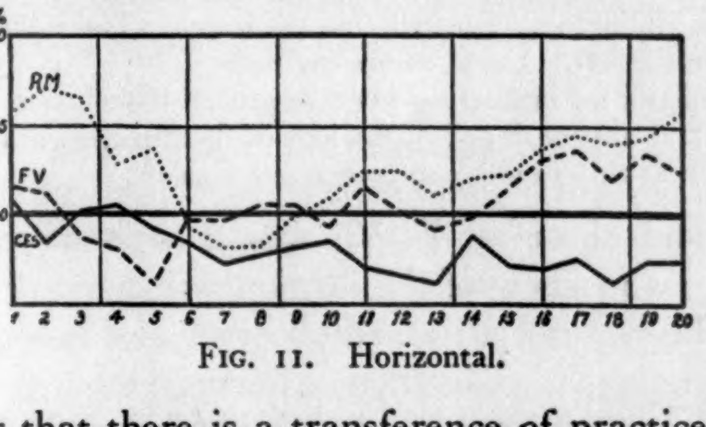


FIG. 11. Horizontal.

It is clear that there is a transference of practice-gain from the conventional form of the Mueller-Lyer illusion to this variant for the illusion is smaller at the beginning of this test than before the training in the former. The amount can not be stated as

the early measurements were made under different conditions. The observer's information of and surprise about, the outcome of the former series does not seem to have resulted in any objective evidence in the results after the day the information was obtained.

There is indeed a considerable illusion for the vertical position and this falls off rapidly during the first three days. The smallness of the error for the vertical position must not, however, be interpreted as evidence of efficiency only but also as the result of unconscious correction for the known motives of the illusion. This is demonstrated by the fact that there is a decided over-correction in the horizontal position. Although the small error in the vertical position remains fairly constant throughout, the series really ends with this also negative, which is further proof of the tendency to correct.

The following introspection was written by C. E. S. after the second day of training:

"The judgment on this figure is very uncertain. I notice three distinct methods of seeing the figure. (1) Seeing the whole figure in one sweep with an attempt to divide it into three parts without superposition of parts. (2) Comparing the diameter with the central space, allowing the limiting arcs of the circle to suggest bands of space about two inches wide, instead of a mere line. (3) Trying to image mere linear distances and superposing these. The third I think is the most effective but it is also the most difficult. The illusion seems to be greatest for the first, then the second is next, and the third is freest from illusion. Wherever the comparison is close I tend to use the third method.

"The circles appear distinctly oblong vertically. The lower circle seems to be larger than the upper. I usually use the lower. The right circle seems to be larger than the left. I tend to use the right.

"I had no basis for estimating the strength of the illusion at the start, if present at all. I am continually conscious of the direction of the illusion and should think that this would lead to a small illusion."

The second observer, F. V., was a sophomore of average intelligence, who knew of the illusion and some of the motives for it; but he was not a trained observer and it was impossible for him to make sharp distinction between what it appeared to be and what he estimated it to be with knowledge of conditions. He started with the illusion practically eliminated and made a very regular and consistent record with practically no illusion present in either vertical or horizontal position.

This record has but little value from the point of view of training. From measurements on variants of the illusion it was demonstrated that this observer was subject to the normal illusion when he was taken unawares. We should not conclude that he was dishonest, nor that he was a poor observer in other respects; but, having satisfied himself about the amount of the illusion before beginning the test, and knowing the tendencies present, he was not able to distinguish the logical estimate from the visual presentation.

The third observer, R. M., was also an undergraduate of strong ability, but he knew nothing of this illusion and remained in a naïve state of mind with reference to it throughout the test. The average illusion for undergraduates under the same conditions in a single test is 15% for the vertical position and 4% for the horizontal. His illusion is therefore 5 per cent above the normal in the vertical position and 1% below in the horizontale To be more specific, with reference to the vertical position he starts with an illusion which is about normal for observers of his class, this remains fairly constant for the first nine days, but in the last ten days, it increases 10%. For the horizontal position, he starts with an illusion above the normal, which falls off gradually to an overcorrection during the first eight days and then gradually returns so that the series of training ends with the same degree of illusion with which it began. The observer was greatly astonished at the results, but he could give no introspective account of changes which would account for these variations in his record.

On the surface the records of C. E. S. and F. V. are similar. C. E. S. undoubtedly reduced his illusion by training on the regular Mueller-Lyer figure but he also made unconscious correction and even over-correction. The first two or three days may also be interpreted as showing decrease with the training in this form. On the other hand F. V. did not have the advantage of practice, was not influenced by any foregoing training but by the knowledge of conditions which resulted in a confessed inability to take the strictly perceptual attitude. R. M. represents a type we have found in every series before—the person who does not know of the illusion and who, as a result, shows

a strong illusion which does not tend to disappear or diminish with practice.

GENERAL CONCLUSIONS.

The essential feature of these experiments lies in the demonstration of a number of factors which determine what effect practice shall have upon these normal illusions. Chief among these are the degree and the kind of knowledge of the illusion, the capacity for maintaining the perceptual attitude, speculative tendencies, the estimate placed upon the known illusion at the beginning of the training, the duration of the training, knowledge of progress, the effect of a long interval after training, and different types of motives for illusion.

The list of the cases in Table XIII is arranged as a partial aid in a review of the results. The Roman numerals refer to the series: I, the illusion of cylinder length; II, the T-illusion; III, the Mueller-Lyer illusion; and IV, the illusion of distance between circles. The observers are designated by their initials; v. and h. denote the vertical and the horizontal positions respectively; s. and p. designate the method of selection and the method of production respectively. 'Strength,' has reference to the strength of the illusion at the beginning of the training; 'Knowledge' has reference to the observer's knowledge of the existence and character of the illusion involved.

The illusion persists with undiminished force so long as the observer has no knowledge of its existence. This is illustrated by Cases I, O. W.; II, D. H.; and III, T. P.—as it was illustrated first in the study on the size-weight illusion and the illusion of the vertical mentioned in the introduction. We have found no exception to this rule.

Among observers who have knowledge of the illusion those who are best capable of maintaining the perceptual attitude (i. e., reporting what they actually perceive as opposed to what they may judge relations to be) are least likely to decrease the illusion by practice. This assertion is based largely upon the introspections and the internal evidence in those cases in which the observers had knowledge. The surest way of aiding the observer in maintaining the perceptual attitude is to find and

keep him ignorant of the existence of the illusion and free from suspicion of it. That was an easy task fifteen years ago, but is exceedingly difficult now in the face of popular knowledge of the illusion.

Of course, if one knows the illusion he can readily learn to make proper correction for it in a judgment. Such correction

TABLE XIII

Case		*Strength*	*Knowledge*	*Practice Effect*
I, C. E. S.,	v.	Small	Full	Not any
	h.	"	"	" "
I, L. F. S.,	v.	Not any	Partial	Overcorrection
	h.	Medium	"	Increase-decrease
I, J. O. D.,	v.	Medium	Partial	Not any
	h.	"	"	" "
I, O. W.,	v.	Very large	Not any	Great increase
	h.	" "	" "	Increase
II, D. H.,	v.	Small	Not any	Slight increase
	h.	"	" "	Slight decrease
II, T. S.,	v.	Medium	Partial, suspicion	Decrease-increase
	h.	Small	" "	Overcorrection
II, C. E. S.,	v.	Very small	Full	"
	h.	Not any	"	"
III, C. E. S.,	s.	Medium	Full	Complete reduction
	p.	"	"	Overcorrection
III, D. S.,	s.	Medium	Full	"
	p.	"	"	Decrease
III, T. P.,	s.	Large	Not any	Not any
	p.	"	" "	" "
III, A. M.,	s.	Large	Partial, suspicion	Decrease
	p.	"	" "	"
IV, C. E. S.,	v.	Small	Full	"
	h.	Not any	"	Overcorrection
IV, F. V.,	v.	Not any	Partial, speculative	Not any
	h.	" "	" "	" "
IV, R. N.,	v.	Large	Not any	Increase
	h.	Small	" "	Decrease-increase

is at first focal in consciousness but soon becomes so automatic that the closest introspection may not trace the correction process involved in the form of an allowance for the illusion. To prove that we can make conscious correction for the illusion would be a waste of energy; to assume that such correction could not be made in a normal individual would be absurd. We

are therefore interested only in the effect of practice upon actual perception, or what seems to be perception; but the judgment process shades so imperceptibly into the perception process that the task of distinguishing them becomes exceedingly difficult.

Some illusions may be eradicated with practice without leaving any conscious trace of the correction. See, e. g., I, L. F. S. v; III, C. E. S.; III, D. S. These are in accord with Judd's results on the Mueller-Lyer illusion and Cameron and Steele's[1] results with the Poggendorf illusion. The figure looks different at the end from what it did at the beginning of the series. But, so far as the records go, such reduction has taken place only for persons who know of the illusion. From this it is not necessary to assume that the decrease is due to conscious correction; it may be due to the ability to avoid a certain kind of eye movements, attention to accessories, etc.

The illusion may disappear at the very beginning of a training series merely as a result of the elaborate and discriminative adjustment for systematic observation, and without any practice. For all who know of the illusion, there is a tendency to begin the training series with a smaller illusion than would ordinarily be shown in a single test. The cases of small, or not any, illusion in the above list illustrate this.

When the illusion decreases as the result of practice, without the appearance of any change in attitude of the observer, the gain takes the form of the conventional curve of learning; the illusion tapers off gradually during one or two thousand trials, with normal fluctuations.

The gain made by training in a series like this is not retained permanently; it may be wholly or partly lost in two years.

Overestimation of the illusion which is supposed to be involved at the beginning of a training series tends to lead to correction. This is one of the clearest evidences of the failure to maintain the perceptual attitude. This is illustrated in I, L. F S. v; II, C. E. S.; and II, T. S., h.

[1] Cameron and Steele; "The Poggendorff Illusion," Yale Psychological Studies, N. S., I, 83.

In partial knowledge of a given illusion, those motives which are known are affected as in full knowledge of the illusion whereas those which are not known remain unaffected by the practice. See Cases I, L. F. S., h; I, J. O. D.; III, A. M.

Suspicion of the illusion is likely to result in erratic records, depending in part upon the rightness or wrongness and in part upon vacillation in the suspicion. Where the suspicion is specific it operates in the same way as knowledge. The increase in an already large illusion may be accounted for by the presence of an erroneous suspicion; e. g., I, O. W., v; and IV, R. M. Case II, T. S., may illustrate a vacillating suspicion with reference to the vertical position and a true and firm suspicion with reference to the horizontal.

Where the same motives are involved, the gain made by training on one variant is transferred to another variant while the results of the training are fresh. See the effect of Series III upon Series IV in the case of C. E. S.

In all these experiments the observers were kept completely ignorant of their records until the training had been completed. With progressive knowledge of one's records the results would undoubtedly be quite different. It would probably be impossible to maintain the perceptual attitude under such circumstances. The gain would, of course, be more general and more rapid.

The fact that observers who are working extensively with illusions tend to have comparatively small illusions would seem to show that the discriminative attitude of the trained observer, regardless of practice in any particular illusion, weakens some motives for illusion and wholly obliterates others. In many illusions trained observers may choose to regard the object in such a way as to obtain a strong illusion, a small illusion, or no illusion at all, at will.

There are undoubtedly two general types of motives for illusion: those which are due to lack of discriminative apperception of the task in hand, as in the large illusion in the T-figure; and those which are due to deeply ingrained misleading tendencies in the interpretation of sensory data, as in the illusion of the vertical. Such complex illusions as the Mueller-Lyer, the size-

weight, the cylinder length, and the T-illusion probably involve both. The force of the rigid sensory motives is also lessened by a keen discriminative attitude of the observer.

The practical value of experiments of this sort is evident. In these training series we have followed the making and unmaking of habits under conditions partly controlled and have traced the dominance and the vanishing of ideas in developing perception. We feel that the possibility of working out a serviceable system of laws governing the persistence and the elimination of normal illusions which enter into our ordinary sensory experience is as promising as it is urgently needed.